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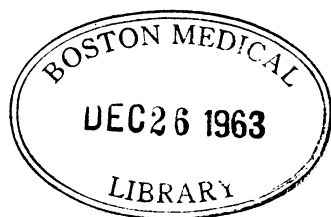
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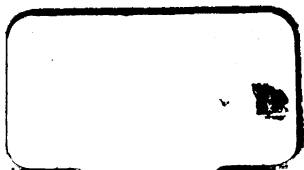


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ORIGINAL INVESTIGATIONS
IN
CATTLE DISEASES
IN
NEBRASKA.

SOUTHERN CATTLE PLAGUE.—THIRD EDITION.

REVISED AND AUGMENTED WITH MANY NEW INVESTIGATIONS, AND THE TRUE
PLACE OF THE TICK AS A VEHICLE OF INFECTION
UNQUESTIONABLY DEMONSTRATED.

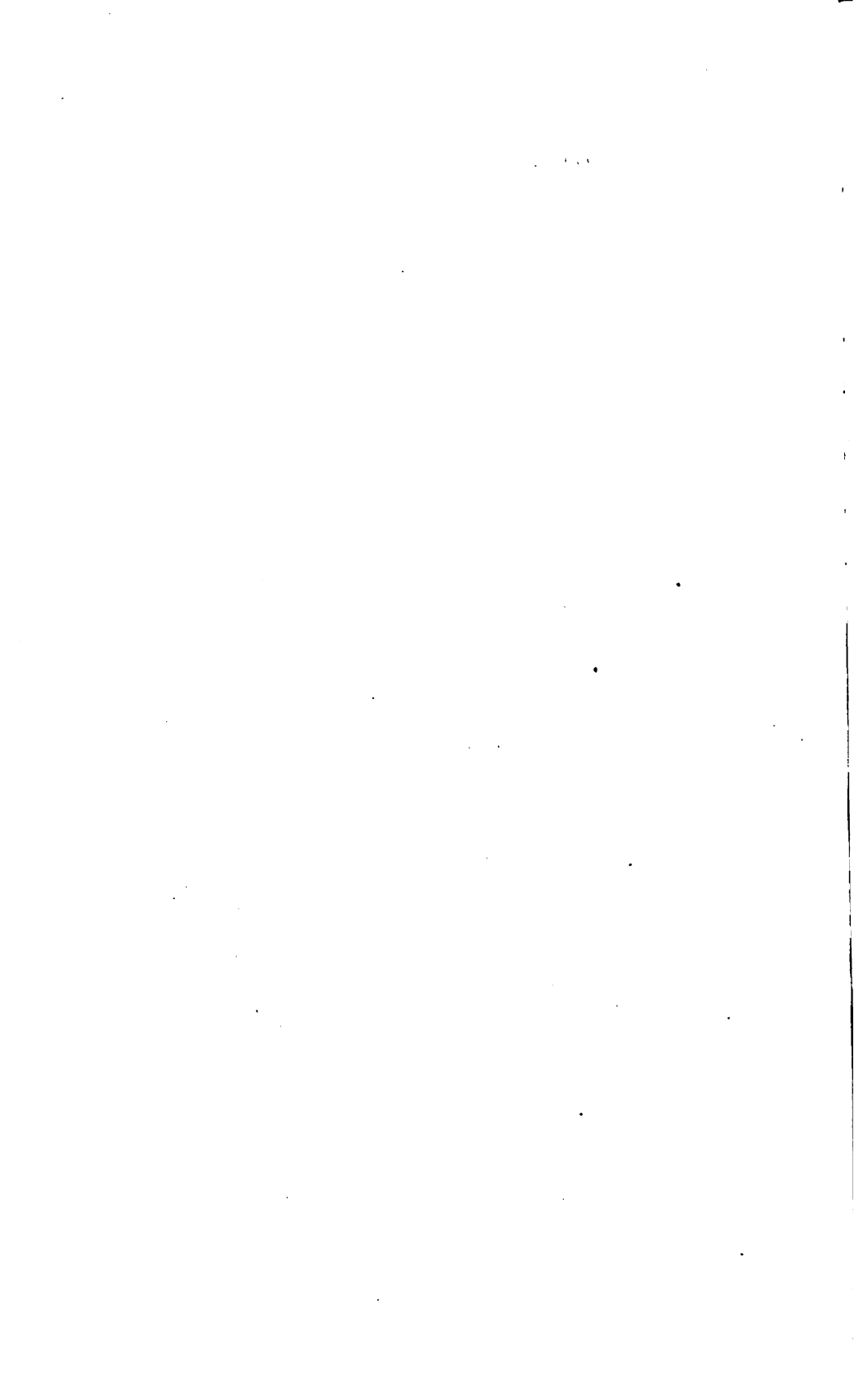
BY

FRANK S. BILLINGS,

Director of the Patho-Biological Laboratory of the University of Nebraska.

ANIMAL DISEASE SERIES No. V.

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THE ETIOLOGY OF SOUTHERN CATTLE PLAGUE— TEXAS FEVER.

BY FRANK S. BILLINGS, DIRECTOR OF THE PATHO-BIOLOGICAL LABORATORY OF THE
STATE UNIVERSITY OF NEBRASKA.

In the year 1888 I published my first investigations in the etiology of this disease, and in 1889 a second edition was published under the title of "Original Investigations in Cattle Diseases in Nebraska," those on the "Corn-Stalk Disease, Hydrophobia in Cattle," and on an infectious disease of the external sexual organs, and of the cornea in cattle having been added. As has thus far been invariably the case, the Agricultural Department in Washington soon after began to throw doubts on the reliability of these investigations, so far as the announcement of a specific micro-organism had been made, and in the case of southern cattle plague to assert the existence of an entirely different organism as the cause of that disease. This was done absolutely on unscientific grounds and utterly without proof. With regard to the "corn-stalk disease," a member of the department has also said, "*I do not take very much stock in the germ theory of this disease, because to accept that we must throw aside pretty much of our experience.*" As the etiology of that disease is now established beyond question, supported as it is by the work of Nocard in France, and Bowhill in England, it is evident that the "stock" which the farmers of the United States and scientific investigators should take in announcements on the etiology of specific diseases, from the National Agricultural Department at Washington, should fall very much "below par." In reality, they must always be taken *cum grano salis*.

That same individual has also said of the Nebraska investigations, "Billings has boldly announced a great many discoveries that have not fulfilled the expectations of his friends." (Reply to Dr. Peters, Journal of Comp. Med., vol. XIII, p. 35, 1892.) *Let me again, with equal boldness, announce my utmost confidence that all my discoveries will eventually fulfill the most exacting expectations of my friends, and*

equally disappoint those who are not, even to inoculation, against swine plague.

I am only too well aware that the unfortunate polemical discussion which circumstances have forced me into has essentially militated against the acceptance of my work, and often prevented an unbiased appreciation of it by other investigators as well as many intelligent readers in the lay public. Still, the general result has been perfectly satisfactory to me as well as those most directly interested, the stock breeders of Nebraska, as is evinced by their insisting on my recall here and my retention in the face of the most bitter and determined opposition of the National Agricultural Department. I was fully aware that my true purpose in this polemic would be misunderstood. I knew full well that European investigators would be a long time in coming to an appreciation of the fact, that dishonest and unreliable work could possibly be put on the public by a department of government. I have in my possession scores of letters from among the ablest men in Europe, asking me, "Is it possible that such things can be?" "Was I sure that I was not mistaken in the matter?" They repeatedly said, "that such things must be impossible; that no investigator, who would do as I claimed, would be retained an hour in the employ of any European government;" which is true.

While European governments undoubtedly are still very imperfect organizations, politics in those countries have not yet degenerated to the machine-rottenness and corruption which is fast threatening republican institutions in this country with disruption or the land with dangerous revolution. We cannot go on in our present course many years longer without one or the other occurring. The despotism of these rotten-machine politics is fast driving reflecting and honest-minded citizens to a condition verging on desperation, without regard to their fortunate or unfortunate position in the social scale. These forces will eventually unite, and then look out. No man can foretell what the result may be. The people are honest at the bottom, though much enslaved at present. Republican institutions are much slower in their movements than rebellion under monarchical governments, but the resulting revolution is liable to be much more radical and far reaching its effects. I have thus plainly stated my opinions in order to show my readers the true spirit which has actuated me in my attack on the work of the Department of Agriculture. To those who know me, to my professors and colleagues in Germany, who know that from the day I began to study, *my ambition has not been to gather honors as a discoverer in the realm of investigation in the branch of science in*

which I have worked, but rather to do the best I could in order to demonstrate the value of scientific investigation to the people of this country, that it might be inaugurated and supported by them through their governments, national and state.

It is well known, that before I came to Nebraska I had given fully as much, if not more, attention to the diseases of human life than of animal, and that it was my earnest hope to have given my entire energies to the study of the infectious diseases of child life. In that I completely failed. Neither the intelligence of any representative body nor the public spirit of any of our citizens were capable of being aroused to any enthusiasm in that direction. Necessity, in one form or another, has been the stimulus to every advance made by the race in its historic evolution. Necessity, the great losses in live stock from disease, was the cause of my call and recall to Nebraska. The necessity of saving the "Almighty Dollar" has thus far been the only stimulus to the investigation of infectious diseases in this country. The necessity of saving a human life from the miseries of disease has not yet made itself apparent to the American people. When it does, the work will be inaugurated and well started. To educate the people in this direction has been the stimulus to, and earnest purpose of, my own work. To attain this end the best I could do was necessary. I am absolutely without ambition for priority as a discoverer, as I am fortunately beyond the necessity of looking to my position for daily support. If I am a "crank" on any one thing, it is for honesty, to a fanatical degree, in the public service. Though a democrat of democrats I know no party, and am absolutely free from party affiliations. "*The good of the people first, the good of the people last, and the good of the people every time,*" is the one and only principle which I lay any claim to in my actions. With such ideas any one can readily see what must have been the sentiments and results when I found the reports of the Agricultural Department, from the year 1880 to 1885, to be absolutely and unequivocally false; to be a tissue of impossibilities so far as they had reference to the specific cause of swine plague, with the exception of the investigations of Dr. J. Detmers, which were claimed to be wrong by the government itself, but were reasonably correct.

With an effrontery that could only come from one hardened with the brazen corruption of an American politician, the Agricultural Department at Washington now tells the people of this country, and defies any high moral character they may have, that the germ of hog cholera (swine plague) was not discovered until late in 1885. "*The first tests in this direction (of preventive inoculation) were made at the*

experiment station early in 1886, soon after the hog-cholera bacillus had been discovered." (Report of Dept. Agr., 1890, p. 110, issued 1891.)

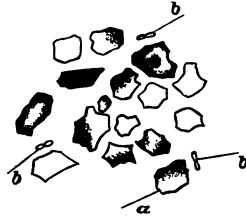
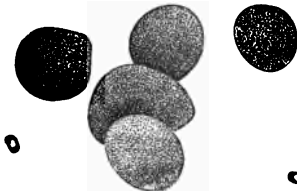
In the report of 1884 that same Department said: "In a former report have (been) given details of experiments; which demonstrate beyond question that the microbe of swine plague (H. C.) is a micrococcus, and the evidence furnished was all that could reasonably be required to decide a scientific question of this kind." (P. 222.)

"In the many cultivations which I have made from material obtained from slaughtered animals I have never found bacilli, except in a very few cases, where the virus was not obtained until after contact with the air, where the vacuum tubes had not been properly sealed, or where the animal was not slaughtered until in the last stages of the disease." (P. 225.)

Certainly these quotations amply justify the severest criticism of a department of government issuing such reports to the public that created it. If the bacillus of hog cholera (swine plague) was not discovered until late in 1885, certainly the micrococcus of 1880 to 1885 must have been a dream, and the "evidence furnished" anything in value than that which was claimed for it. The most exact and unbiased search over the experiments reported by the government in proof of the fact that the germ of swine plague (H. C.) was discovered and demonstrated as such by them will utterly fail in revealing that fact. They stand to-day in no more favorable position as actual demonstrators of that germ than did Dr. Detmers in 1880. There is not an iota of proof that they discovered the germ even (except they say so) in the report of 1885.

It has been said that my denunciation of the germ of 1885 as a "forgery" is false. A "forged" note, though false in description, may bring the money at the time. Let any one who has carefully investigated this question, and stained the bacilli of this disease, compare the illustration and description given in the Agricultural Report of 1885, p. 212, "*the darker portion is not localized at the two extremities as in the bacteria of septicæmia in rabbits, but is of uniform width around the entire circumference of the oval,*" with his own observations and then with the illustration, "Fig. 2" (comparing Fig. 2 with Fig. 1) of plate (Special Report on "Hog Cholera," 1889), and say whether that description and illustration in report of 1885 is false or not. Let him remember that in 1885 the government distinctly says, *the darker portion is not localized at the extremities as in the bacteria of septicæmia in rabbits.*" What then can any unbiased and honest man

say, who, in 1886, and since then, reads that it is stained at the extremities as in the bacteria of rabbit septicæmia, and is illustrated as so staining?



Germ of 1885.



Germ of 1886.

When does a man receive credit for the discovery of a germ, when he gives a description by which no one can recognize it, or when he gives a correct one with a recognizable illustration?

The latter is the accepted view, as everyone must admit! Then according to that, instead of discovering the true

germ of swine plague late in 1885, the government did not discover it until 1887, because, as is the universal custom, the report of 1886 was not issued until late in the spring of 1887. All I ask of the scientific world is the customary verdict in all such cases. I did not discover the germ of swine plague first; Dr. Detmers did that. All I ask, in the name of common honesty, is justice to all. Do not forget that even now it cannot be shown from the reports that the government investigators have exactly and unquestionably demonstrated by scientific experiments that the germ of hog cholera, as they now call the real swine plague, is the actual cause of that disease.

Next, in order to militate against the value of preventive inoculation, the Agricultural Department announced a new and widespread swine plague in its report of 1886, but up to to-day has utterly failed in accurately demonstrating that a germ, accidentally found in swine (when diseased with the real plague) and other animals, whether diseased or not, even causes disease of any kind, *of itself*, as a natural infection in swine or other animals.

The reader may ask himself, "Why on earth is Billings thrashing all this old straw over again?"

Merely to show the absolute unworthiness of belief in any assertion of the Agricultural Department regarding a specific germ in causing any disease, unless that assertion is supported by the investigations of

men known to be honest and reliable investigators. The reliability of any statement must always largely depend upon the record of those making it for trustworthiness, unless we absolutely know it to true from our own observations.

As for the past six years, I have been most bitterly attacked, which attack seriously reflects on the honor and intelligence of the people employing and supporting me, and as these attacks have lately had the personal support of the Secretary of Agriculture, I now desire to place before the world all the testimony known to me referring to the acceptance of the government's position, by those who have given an intelligent consideration to the question.

THE AUTHORITIES ARE ALL AGAINST THE RELIABILITY OF THE
GOVERNMENT'S REPORTS.

In his letter to Senator Paddock (since published and scattered broadcast by the Department) the Secretary of Agriculture says of Dr. Billings:

"He finally demanded a commission of scientific men to decide whether or not he had shown the investigations of the Bureau to be unreliable, whether the reports of the Bureau were true or false, and whether, to use his own words, he himself was or was not a fraud. On the request of the National Swine Breeders' Association, such a committee of scientific men was appointed, * * * it was constituted according to Dr. Billings' wishes; it was pronounced by him to be satisfactory, and yet, after a long and careful investigation, it endorsed the investigations of the Bureau of Animal Industry in every essential particular, and in every essential particular decided that Billings was wrong, and intimated that his methods of work were not all that was to be desired. Not satisfied to allow the controversy to drop here, he issued a pamphlet entitled 'Evidence Showing that the Report of the Board of Inquiry Concerning Swine Diseases Was Fixed,' in which he attacked the honesty and veracity of the honorable scientific gentlemen composing the commission."

This letter to Senator Paddock should be published in full, as, if correct, the writer of this article is indeed a fraud, not only as an investigator, but on the people of the United States and the world in general, but it will be deferred to an article on inoculation.* This is not a personal quarrel by any means. On my part it is a bitter bat-

* See the author's work entitled "A Public Scandal," 1892.

tle for honesty in public office; a desperate fight against machine politics and its corrupt officials, and, so far as I can, I mean to expose every iota of it to public view. The following criticism on the Report of the Board of Inquiry most certainly demonstrates beyond all question that the Hon. Secretary of Agriculture, Mr. Rusk, has been imposed upon, and that the verdict of the world is directly contrary to what he thinks it is.

THE OTHER SIDE OF THE QUESTION.

"The Agricultural Department of the United States named a commission, composed of Shakespeare, Burrill, and Bolton, to settle the question between Salmon and Billings in connection with the pestiferous diseases of swine in the country, and to make proper investigation, and report on the same. The commission could not complete the task in the time given them, which fact they mentioned in their report. Nevertheless the government published the report, and accepted its conclusions. The individual experiences of the commission in the diseases in question are small, especially insufficient, and of extremely problematical value are they in relation to the 'swine plague' (Salmon's) or infectious pneumonia. Notwithstanding the assertion of the commissioners, that they based their conclusions on personal observations, and notwithstanding certain positive statements in their 'conclusions,' the report gives the impression of uncertainty from want of certainty, and a sufficient scientific foundation. The conclusions appear, therefore, to be statements which have only the worth of insufficiently founded suppositions and their correctness cannot be substantiated except with difficulty. * * * As to the priority of the discovery of the germ, and the quality of the scientific work, the commission took the side of Salmon as against Billings, but also took care to mention that the government could go on with the work without any outside help. Billings' preventive inoculation finds acknowledgment between the lines." (*Jahresbericht ueber die Fortschritte in der Lehre-Pathogenen Mikro-organismen*, Baumgarten, 1889.—Annual Report on the Progress in the Study of Pathogenic Germs, 1889, p. 178.)

In remarks on some of Billings' publications while in Chicago the succeeding *Jahresbericht*, 1890, says:

"These and other things form the introduction of a pamphlet (by Billings) against the assertion of the swine plague commission that his methods were not correct. He justifies the methods that he used, the spleen of swine, to gain cultures from, and demonstrates that it was

carefully done, so that it appears as if no possible objections could be raised against it. Billings blames, apparently with justice, the commission for treating the subject in a very superficial manner, so far as his laboratory and work were concerned." (P. 185.)

In a very careful and critical review not only of the American swine plague literature, but also a detailed comparative study of the germs of the true American swine plague (not Salmon's) with those of swine plague (European) and other diseases of Europe, Dr. P. Frosch, assistant to Robert Koch in the Laboratory of Hygiene, Berlin, Germany, published his conclusions as follows:

"1. The bacterium of hog cholera (Salmon) and swine plague (Billings) are identical.

"2. The same is the cause of the American swine plague, while the proof of the etiological (causal) connection with this pest, of Salmon's germ of swine plague, that is, for the existence of a second ('widespread epizootic disease') plague of equal extent is not sufficient to warrant such a conclusion." (*Archiv fuer Hygiene*, vol. 9, p. 279.)

On page 247 of the same journal, and in the same article, Frosch says:

"All these circumstances (previously considered in detail), that is, the appearance of cholera and swine plague (Salmon) in the same animal and the same outbreak, and the same time, the presence of still other germs in the organ of the diseased animals, and the fact in such cases reference is made to chronically diseased hogs, as well as the results of the inoculation experiments, justify the assumption that the swine plague bacterium of Salmon is an accidental presence in chronic cases of hog cholera, for which any idio-pathogenic relation to another pest sufficient evidence has not yet been produced."

As the members of the Swine Plague Commission saw fit to condemn the methods by which they supposed I worked all the time, and did say I worked with at the time, as they brought the best practical results, it may be well to state what an entire outsider has to say on the methods of investigation as published by the Bureau of Animal Industry in its own reports. On this point Frosch says:

"The most important results must be those obtained in swine by the inoculation of undoubtedly pure cultures of the hog cholera (S.) bacterium, because in that way only can the specificity of the organism be proven. *But it is directly in this relation that one meets with essential insufficiencies (in the government work).* Notwithstanding the numerous experiments made by Salmon with pieces of organs and heart's blood, etc., of diseased swine, still those made with pure cult-

ures of the germ are made prominent by their scarcity and generally negative character. The small number of these positive results would not call forth any objections as to their value were it not that they did not fulfill all those conditions which are essential in deciding questions of this kind. Other experiments detailed in reports of 1885 and 1886, *there is not one which can be said to be free from objections.* The cause of the same lies, first, in the fact that the order of the experiments was not correctly arranged, the control animals dying first in two cases and at the same time in a third; while in the feeding experiments of the same year control animals are not mentioned. A second very essential objection to the method pursued by Salmon is the manner he went to work to obtain pure cultures, * * * *the consequences of such an unscientific method are to be seen distinctly in the report itself*, when, mixed with the inoculated germs other 'large' or 'fine' bacilli are mentioned as being present. The best proof of the unscientific character of his method is given by Salmon himself in the assertion that cholera and swine plague can occur in the same animal, a connection which absolutely demands in each case the use of the plate method." (*Ibid*, p. 243.)

To this detailed study and exact report of Dr. Frosch's the government investigators most seriously objected, as would be natural, and one of them made reply thereto in the same Archiv (*Zeitschrift fuer Hygiene*, vol. 10). To the same Frosch answered as follows:

"The foregoing article by Dr. Theobald Smith, and especially his comments on a contribution of my own, induces me to once again place my position regarding the American swine plague clearly before the world. To begin at once with the point that seems to have mostly irritated Smith, I do not think that any one but he can find in my former article any special partiality or unjust discrimination in favor of the investigations of F. S. Billings. Such an estimate of my contribution to the question is only possible to a person who has cursorily read the same, or who is profoundly ignorant of the character of the hygienic institute at Berlin, and the work which is done therein.

"As I declared in the beginning of my previous article, the reception of the cultures from Billings at this institute, and the request of Prof. Koch, led me to enter upon the study of the American swine plague. The task which I had to undertake was not, as Smith appears to believe, to decide as to whom belonged the most or earliest credit for work done, but to see how far, from a purely scientific point of view, the solution of the real question of the etiology of this disease had been

advanced, which, at the time, seemed to be buried in darkness, in consequence of contradictory publications.

"That I should depend more upon my own investigations with the cultures at my disposal than upon the investigations of Billings should not be questioned by Smith. That I should refer to the investigations of Billings, after assuring myself of the identity of his germ and Salmon's hog cholera germ, in order to decide as to the pathogenity (disease-producing power) of the germs in swine, was forced by Smith himself, *for, as much as I regret to have to repeat it, the methods and experiments published in the reports of the Bureau of Animal Industry (1885 to 1887-8) do not correspond to the scientific conditions necessary to the establishment of a new infectious disease in a manner to be desired.*

"It is by no means necessary for me to repeat what I said in my previous publication, as Smith admits it, in regard to the report of 1885, and, on the other hand, the superficial investigations described in the other reports display so little exclusion and exact employment of Koch's methods to correspond with the importance of the assertion of the appearance of a new exciter of infection (germ), closely related to the swine plague.

"I might here call attention to the fact that even in the work of Smith, described in his article in this issue of the *Zeitschrift*, the method for differentiating the two germs, the employment of the hanging drop, is not sufficiently reliable.

"Smith also seems to complain that I have considered the mentioned reports of the Bureau of Animal Industry too closely after the standpoint of to-day. Even though we may have to-day new ideas of what a pure culture should be, or substantially other methods of obtaining the same than formerly, still it was perfectly justifiable to prove the case as to how the earlier results of the Bureau correspond with these newer ideas.

"As shown in my previous publication, it is evident from the report of the bureau that at that time Koch's methods were well known there, and I do not think that it is demanding too much of the bacteriologists of the bureau to assume that they are acquainted with the methods as published in their reports.

"As to the slur upon Billings' work in Smith's publication, I can safely leave it to the former investigator to consider them. I have only to refer here to his inoculation experiments in swine, to which I am inclined to give full credit as reliable evidence, because Billings emphasizes the control of the pure cultures used in the same by other cultivating tests.

"The number of these experiments was sufficiently great to demonstrate the infectiousness of the germs and their specific characteristics. Smith neglects to observe that the experiments quoted by Billings were specially selected out of a great number. I find it remarkable that Smith should question these twelve published experiments of Billings, the value of which is beyond doubt.

"The inference (by Smith) that I allowed Billings' publication to influence me in the consideration of Salmon's swine plague, by no means corresponds with the facts. *For judging this question I have referred to the publication of the Bureau on the assumption that in these official reports the most reliable material must be found.*

"In regard to the question as to whether the Salmon swine plague germ is an independent cause of a specific disease, I cannot change my previously expressed opinion. How far I am right can best be judged by reading Smith's publication in this journal.

"As will be remembered, Salmon, supporting himself on the German Schweine seuche, distinctly and emphatically asserts the existence of an independent plague, and at the same time united with the hog cholera, which had the same degree of extension over the country.

"Judged by the investigations published in the reports of the Bureau, we find but proportionately few cases, and these not free from objections, of the appearance of a disease-producing germ in chronic cases of hog cholera. The conditions closely resemble those seen in certain infectious diseases of man, where the secondary appearance of pathogenic germs has long been observed, without any one asserting them to be independent causal moments, or the cause of extensive epidemics.

"The last two cases reported by Smith in favor of his swine plague do not give evidence which sufficiently excludes the concomital existence of hog cholera also. At the same time and the same place, several swine are reported to have died from the cholera.

"The fact stands for me confirmed, that in the five years that have passed since the second plague was first announced not one single case of an independent appearance of Salmon's swine plague has been reported which can be said to be free from objections.

"The mistaken conception of Salmon's place in the swine plague investigations must be laid to the fact that the publications, both on swine plague and hog cholera, have his name, and as such have been quoted in the literature.

"From the present publication of Smith's, however, which could not be seen in reading the reports of the Bureau of Animal Industry, it is

evident that Salmon was not the discoverer of either the hog cholera germ or that of the swine plague, so now we know the true condition of things in that regard."

An American reviewer of the entire question, so far as it pertains to the publications of the Agricultural Department and the report of the Board of Inquiry, says:

"We have compared the extracts cited by Dr. Billings from the reports of the Department of Agriculture, line by line and word for word; we have carefully examined the plates accompanying these reports to which he refers, and we have also read the entire context from which he quotes, in order to avoid any possible bias which may follow from reading disconnected sentences. As a result we are enabled to say that all of Dr. Billings' charges, sweeping and severe as they are, are true and just, and we cannot understand how the committee appointed to investigate the special question involved could fail to make the same examination, to arrive at the same result, and to publish that result in calm, judicious language, not as a matter of justice to Dr. Billings, or of criticism of Dr. Salmon, but as a positive duty to science. Regarded from this point of view, the report of that committee is the most disappointing document of the kind we have ever seen. This we may say, without intimating, as Dr. Billings does, that its authors were under the control of the 'Bureaucratic Whip.'

"The charges made by Dr. Billings, expressed in more conventional language than he employs himself, are that Dr. Salmon's bacteriological work is not expert, that he described a germ of swine plague when he had no such germ in his possession, that he has printed assertions without scientific evidence to sustain them. He even states directly that one micro-organism (the description given by Dr. Salmon) was evolved from the inner consciousness of that gentleman, and designates this procedure by a term to be found in the criminal code, and not usually employed in scientific discussion, however applicable it might be regarded in one sense to evidence, which Dr. Billings states to have been deliberately made up for the occasion.

"Regarding the bacteriological questions involved, we would be inclined to suspend judgment, were it not for two sets of facts: First, the evasions of the 'Board of Inquiry'; second, the gross logical errors and inherent contradictions of Salmon's writings; the latter are glaring; it would not have hurt Dr. Salmon's reputation, had he, on being convinced of errors in his methods or conclusions, candidly acknowledged them, and accepted the corrections furnished by others and verified by himself. Men of a real rank in the scientific hierarchy, like Virchow,

Klein, Koch, and others—and Dr. Billings here sets his opponent an example worth imitating—(No. III, pp. 50-53)—have not hesitated to acknowledge mistakes, such as the most honest and able investigators are liable to in experimental science, as well as in interpreting or quoting the results of others. But Dr. Salmon has preferred to gloss over the matter, and in order to sustain a claim to priority at the expense of another, and to conceal the fact that he adopted the corrections of others, has involved himself in a labyrinth of contradictions; the inevitable consequence of misrepresentation, be they intentional or unintentional. Which of the two classes Dr. Salmon's belongs to, no one who reads Dr. Billings' pamphlet and verifies its citations can for a moment doubt." (*Journal Comp. Med.*, vol. X, p. 399.)

Again the same reviewer says:

"Regarding the bacteriological side of the question, the commission is equally unfortunate, and although the majority report claims that on the substantial point a dissenting minority report by Prof. Bolton is in accord with their own, we do not think our readers will agree with them. Prof. Bolton says:

"*During my work as a commissioner I have failed to meet with an epizootic which I am satisfied was what is termed 'swine plague' in the Bureau reports, though previous to my appointment on the Board I studied one such outbreak.* In this case, however, I directed my attention to the bacteriological questions exclusively, and I am therefore unable to pronounce on the difference in the pathological lesions in the two diseases. But I am not inclined to attach any great importance to these differences as set forth in the reports. The description otherwise I find correct and well stated. In my investigations as commissioner I have been able to find but one organism which in my opinion caused the outbreaks under examination, and that I regard as identical with the hog cholera germ described in the reports of the Bureau, and I find the description therein given correct. As will be inferred from what has gone before, I feel sure that another organism, correctly described in the reports as the "swine plague germ," is found under circumstances which render it highly probable, if not certain, that it also causes disease. As to whether these two organisms are always present and operate together to cause disease, or whether the two are merely varieties of the same germ, must be decided by future investigation."

"The main report insists that there are at least two widespread, distinct diseases which are caused by distinct micro-organisms. If this is what the committee calls agreeing, we would like to know what disagreement is. Who will be in doubt for a moment as to who, or

what induced Prof. Bolton to write the last few lines of his report. They are merely loop-holes for the escape of the man through whose hands the report went. Whether pity or courtesy were Prof. Bolton's motives, he owed a higher duty to the country than to Dr. Salmon; we regret that he was not less ambiguous in expressing his convictions, although as contrasted with the majority report we have occasion to be grateful for even this much."

"FOOLING THE SWINE BREEDERS."

There is a well-developed and continually growing belief among the great body of American swine breeders that they have been badly fooled recently—in short, they are commencing to look upon that meagre report of the so-called special commission on diseases of swine as a fiasco of the first water. 'Twas with not a little satisfaction that they regarded the appointment of the said commission. Were high-toned scientists to be relied upon, the material of the Board was surely satisfactory, and an appropriation of \$30,000 for their expenses seemed ample to produce tangible results.

When the august body got into line of work December, 1888, swine breeders the country over were perfectly willing to wait until April 1 for the momentous findings expected. When April fool's day came they were, according to the custom of the day, fooled. The report came not. Premonitory rumblings of the approaching verdict were now and again heralded through the press. The mountain of scientific knowledge—the Parnassus of bacteriological learning—travailed in labor and brought forth—a mouse! The insignificance of the commission's production, so far as its intrinsic value to a long-suffering fraternity of breeders was concerned, was certainly in the proportion indicated, and any iota of value it may have contained was destroyed by the deceptive method of its publication.

A "boiling down" editorial bureau had been established at Washington and the commission's report was probably the initial work it edited. The press of the country looked for assistance from the said editorial bureau, and prepared itself implicitly to receive and publish as official and authentic the edited advance proofs the said Bureau was paid to disseminate. Thus when about the second week of August a publication purporting to be the true and correct finding of the swine commission arrived at Washington, it was immediately published in full and commented upon as the *in extenso* report. The *Farmers' Review* was not fooled, but in a short editorial outlined the weakness of

the report, and since than has had no occasion to change its assertions. They will, however, bear considerable elaboration.

The Department of Agriculture editor, or mayhap some one more closely interested in the verdict of the commission, sent out the preliminary portion of the report with an explanatory introduction in which occurred the following statement:

"We subjoin here the conclusions attached to the report of the commission, and signed, owing to the absence of Prof. Bolton, by Dr. Shakespeare and Prof. Burrill. Prof. Bolton, however, *furnishes a supplementary report practically confirming the report of his two colleagues.*"

The italics are our own, used to emphasize the assertions that Prof. Bolton's report confirmed that of the other commissioners. We shall see whether that was the case; but first let us ask, why, believing this, Prof. Bolton's report was not sent out at the same time with the conclusions signed by Professors Shakespeare and Burrill? That it was not, leaves the swine breeder to accept the unpleasant inference that it was held back to give the conclusions the most favorable character as concerning Dr. Salmon and his Bureau of Animal Industry.

This subterfuge has, however, proved unavailing. It has been recoiled in hurtful strength upon those who employed it. The ire of the breeders and of the bulldozed press has been aroused since receiving the official report in full, of which Prof. Bolton's contribution is perhaps the most important portion. The result must and certainly will be an unanimous demand for clean work in high places, for just value for public money spent, and for actual, honest, practical research in behalf of the inestimably important swine industry of this country.

As mentioned in a previous editorial in these columns, the commission's conclusions, as sent out by either the editorial bureau at Washington, Dr. Salmon, or the Bureau of Animal Industry, declare that there are two prevalent diseases of swine in this country, *i. e.*, "hog cholera" and Dr. Salmon's "swine plague." The former disease has in all conscience been bad enough. Our swine have had a sufficiently damaging character given them as regards disease. The industry has been hurt thereby. Germany, for instance, has barred out our pork, presumably on account of disease. The patriotic authorities at Washington surely forgot these things when they, with all their power of official advertising, solemnly declare that another serious disease is rife among our hogs. The breeders will thank them for this, doubtless. They will regard their \$30,000 well spent in accomplishing such results. They will appreciate government aid in fostering their interests. They

will admire the self-conceited confidence of a commission which, nothing daunted, proclaims to the world a new disease among our hogs without offering one solitary fact to substantiate the baneful assertion. Time will show what they really think.

And Prof. Bolton confirms this advertising of "swine plague," does he? Let us see. A comparison of the "conclusions" of Profs. Shakespeare and Burrill with the independent minority report of Prof. Bolton, *in re* "swine plague," will enable the swine breeder to arrive at a correct conception of the truth of the matter. (See next page.)

If these two reports corroborate each other we fail to see it, and we feel sure that breeders find themselves in the same position.

In addition to getting up this wholly unwarranted "swine plague" scare, the commission seeks to throw cold water on legitimate and praiseworthy attempts to prevent "hog cholera," by inoculation with carefully prepared virus of the disease. Totally unable to destroy pigs thus inoculated, either by exposing them to an outbreak of cholera, or by feeding them with germ cultures, it warns breeders against inoculation as a preventive, on the grounds that it may tend to spread the disease. The points made against inoculation are those long recognized and time and again written about by Dr. Billings, who considers them of little moment if proper precautions are taken in carrying out inoculation.

The fact that the inoculated pigs from Nebraska proved immune against hog cholera, as demonstrated by the commission, is practically the only valuable thing given to the public in return for the \$30,000 expended.

The swine-breeding fraternity cares little or nothing about the *pros* and *cons* of learned squabbles about germs. It is interested in finding some method of preventing or alleviating the heavy annual loss from swine disease. Naturally enough it has looked to the Bureau of Animal Industry for help in this direction, but for the thousands upon thousands of dollars spent presumably in their interests they have reaped not a single *original* fact of real practical value in their everyday business. Time, indeed, that the work of such a Bureau were thoroughly inquired into! Or, better still, that the government should recognize the fact that little but unintelligible bulletin publishing has hitherto been accomplished, and that the time has arrived when practical work is imperative.

CONCLUSIONS.

It is the opinion of the commission, based upon their own individual observations and examinations of the subject, that *there are at least two widespread epidemic diseases of hogs in this country* which are caused by *different micro-organisms*, but which have a clinical history and pathological lesions more or less similar and very difficult to distinguish without the aid of the microscope, and resort to bacteriological methods. * * * So far as the knowledge and the observation of the commission go, one of the epidemic diseases, viz., that called by the Bureau authorities "swine plague," appears to be far less prevalent than the other which has been named by them "hog cholera." The commission are further of the opinion that the disease called by the authorities at Washington "hog cholera" is caused by the specific action of a certain microbe named by them "the hog cholera germ," which has certain characteristics of form, size, movement, mode of growth in artificial cultures, and action upon certain lower animals, and taken together enable one to distinguish it from the other microbes which have been described from time to time by various authorities as present in swine disease; and that the descriptions of this microbe and its peculiarities, as set forth in recent annual reports of the Bureau of Animal Industry, are fairly accurate. The commission are of the opinion, *although to a less positive degree*, that the epidemic disease called by the Bureau authorities "swine plague" has as its specific cause a certain microbe possessing characteristics which have been fairly well described in recent annual reports of the Bureau of Animal Industry, which distinguish it both biologically and pathologically from the first mentioned "germ of hog cholera."

PROF. BOLTON'S REPORT.

During my work as commissioner *I have failed to meet with an epizootic which I am satisfied was what is termed "swine plague"* in the Bureau reports, though previous to my appointment on the Board I studied one such outbreak. In this case, however, I directed my attention to the bacteriological questions exclusively, and I am therefore unable to pronounce on the difference in the pathological lesions in the two diseases. But *I am not inclined to attach any great importance to these two differences* as set forth in the reports. The descriptions otherwise I find correct and well stated. In my investigations as commissioner *I have been able to find but one organism* which in my opinion caused the outbreaks under examination, and that I regard as identical with the hog cholera germ described in the reports of the Bureau, but I find the description therein given correct. As will be inferred from what has gone before, I feel sure that another organism, correctly described in the reports as the "swine plague germ," is found under circumstances which render it highly probable, if not certain, that it also causes disease. As to whether these two organisms are always present and operate together to cause disease, or whether the two are merely varieties of the same germ, must be decided by future investigation.

Let the swine breeders of America unanimously demand such work. Let them hang no longer upon the words of false prophets or consent to be blindly led by the blind. Write off as lost the vast sums of money already expended in the research of swine diseases, and commence anew, and that now. The Bureau has demonstrated its inability under its present head to serve the swine-breeding industry. Commissions are evidently a delusion and a snare. Other means and methods must be employed, and it is for the swine breeders of the country to demand the change in no uncertain voice.

Let the best men to be found the world over be employed forthwith, if possible, not as special commissioners, but each in his individual capacity, to investigate swine diseases in the different parts of this country. Let each man work for his own honor at the expense of the government, and offer them inducements for the discovery of disease cures or preventives. In this way good results will speedily be arrived at in place of voluminous and valueless bulletins offered to the long-suffering public in place of tangible duties performed at the public expense.

The *Farmers' Review* has come to the conclusion that swine breeders must put a stop to all this fooling they have been subjected to and demand and obtain the government aid in regard to swine diseases, which is their right. This matter is one which demands united, energetic, enthusiastic agitation. Let the breeders strike while the iron is hot.—*Farmers' Review*, Chicago.

"A QUESTION FOR THE COMMISSION."

"The Report of the United States Board of Inquiry Concerning Epizootic Diseases Among Swine"—the conclusions of which were submitted last week—has been received.

The letter of instruction to these gentlemen, signed by Commissioner Colman, was unmistakably drawn to direct the investigation of the scientists to the points at issue between the Department of Agriculture on the one hand in the persons of Dr. Salmon and his assistants, and the Universities of Nebraska and Ohio on the other as represented by Dr. Billings and Dr. Detmers. The commission was specifically charged to determine whether the work of the Department was accurate and original, both of which points were in controversy raised by Drs. Billings and Detmers. That done, independent investigations were to be undertaken. The disputes between the doctors named, who have been engaged in this line of investigation, involved several points in bacteriological research—with which the *Gazette* and other laymen have

nothing to do—and a vastly more important issue—namely, whether there are one or two distinct swine plagues. The Department affirmed there are two—the one “hog cholera,” with its specific germ and the seat of the disease in the intestines; the other, which is named “swine plague,” with a distinct germ, the resultant disease of which finds lodgment in the lungs. The other investigators denied the existence of more than one disease and one germ, claiming that the work of this pest germ was at times manifest in the intestines, at others in the lungs, according to certain conditions.

Such was the chief subject of dispute. As stated in our last issue, on all points raised the commission has determined in the Department's favor. *And yet there is an indefiniteness about its report that is very unsatisfactory.* It declares that there are two widespread epidemic diseases of hogs, and that their descriptions by the Bureau are fairly accurate, “except it does not appear that ‘hog cholera’ of these reports can be said to have its special and exclusive seat in the digestive tract of the animal as distinct from the lungs.” In view of all the circumstances this appears to be a very significant exception. “Swine plague”—the “new” disease—is pronounced far less prevalent than “hog cholera.” After confirming the Department's conclusions as to “hog cholera” and its germ, the commission turns its attention to the other diseases thus:

“The commission are also of the opinion, *although to a less positive degree*, that the epidemic disease called by the Bureau authorities ‘swine plague’ has as its specific cause a certain microbe possessing characteristics which have been fairly well described in recent annual reports of the Bureau of Animal Industry, which distinguish it both biologically and pathologically from the first mentioned ‘germ of hog cholera.’”

Is this the language in which science affirms its conclusions—“to a less positive degree”? Again, Dr. Bolton, the third member of the commission—who files a supplementary report—submits the following:

“During my work as commissioner I have failed to meet with an epizootic which I am satisfied was what is termed ‘swine plague’ in the Bureau reports, though previous to my appointment on the Board I studied one such outbreak. In this case, however, I directed my attention to the bacteriological questions exclusively, and I am therefore unable to pronounce on the difference in the pathological lesions in the two diseases. But I am not inclined to attach any great importance to these differences as set forth in the reports. The descriptions otherwise I find correct and well stated. In my investigations as commissioner I have been able to find but one organism which, in my opinion, caused

the outbreaks under examination, and that I regard as identical with the hog cholera germ described in the reports of the Bureau, and I find the description therein given correct. As will be inferred from what has gone before, I feel sure that another organism, correctly described in the reports as the 'swine plague germ,' is found under circumstances which render it highly probable, if not certain, that it also causes disease. As to whether these two organisms are always present and operate together to cause disease, or whether the two are merely varieties of the same germ, must be decided by future investigation. The differences between them, as pointed out by the Bureau, are sufficient to compel us to treat them as different germs, however perplexing it may seem that two micro-organisms are capable of producing such similar or, it may be, identical lesions."

Be it observed that although Dr. Bolton has not in all his investigation the country over as commissioner discovered one single case of this second disease—"swine plague," he yet "feels sure" that another germ is found under certain circumstances which render it "*highly probable*, if not certain," that it causes disease! On what evidence is he "sure" that it is "highly probable"? Is this again the language with which scientists seek to saddle a *second* swine plague on the country? The *Gazette* cannot be charged with hypercriticism of scientists, but it confesses it is unable to appreciate the force of Dr. Bolton's statement of the situation.

In fact, the definiteness of the commission—"to a less positive degree"—as to the existence of a second swine plague, should have led it, the *Gazette* believes, to submit conclusive evidence to the country on this point. That a germ has been found which the department believes is the cause of its "swine plague," does not admit of doubt; the question arises, however, is this the germ of merely a local, non-infectious, non-contagious disease, or is it a genuine *pest*? If the commission has answered this query conclusively, the *Gazette* has failed to observe it. "Hog cholera" is admittedly a plague; its germs, fresh from the blood or cultivated in artificial media, infallibly produce the disease when introduced into the systems of healthy swine. If "swine plague" is a pest, its germ should likewise produce that disease with its specific lesions—but as we read the commission's conclusions, it can scarcely be said to have specific lesions. Now the commission had in its charge, since February last, nearly twenty hogs, which, in Nebraska, had been inoculated against "hog cholera," and they withstood every attempt made by the commission to kill them by exposure to virulent natural outbreaks, or by inoculation with excess-

ive doses of the germ of "hog cholera." They were "hog cholera" proof, as the commission admits. But *these hogs were not tested with "swine plague" germs or exposure.* And the question which the *Gazette* respectfully asks the commission is, Why not? Here was the opportunity of all to demonstrate beyond all cavil the existence of a second *pest*, for if these hogs had succumbed to inoculations of "swine plague" germs, Dr. Billings would have been proved conclusively a charlatan, an ignoramus, and a fraud on his own chosen ground. He defied the commission to kill his inoculated hogs with any contagious or infectious swine disease; it could not kill them with "hog cholera"; if a second pest—"swine plague" exists, why was not its existence proved thus undeniably? If it kills swine as a *pest* these hogs should have succumbed to it, as they had not been given immunity from its germs, and the commission was in possession of the material with which to infect and kill them if it could. Why was this not done?

The *Gazette* asks this question in all sincerity. It has the utmost respect for the scientific attainments of the members of this commission, and it cares not one picayune whether this or that investigator goes down in the final determination of this much-discussed question; it is interested solely in learning the *truth*; but when it is asserted that two swine pests exist, the matter of the prevention of the disease by inoculation becomes necessarily so complicated as to be almost, if not quite impossible of attainment, and before that conclusion is definitely reached, swine breeders have a right to demand unconditioned proof. Why was it not offered when the opportunity presented?—(*Breeders' Gazette*, August 21, 1892.)

HOG CHOLERA.

The following investigations were begun in November of last year, and have been continued up to the present time. We were prompted to undertake them, not only on account of the scientific interest involved, but also on account of the news of many disastrous outbreaks of the disease in various parts of the state. In the course of our work we have visited many widely separated portions of the state, and encountered the disease in all its stages. In some herds the attack had just commenced; in others we saw only cases of long standing, and, as will be seen elsewhere, the disease presents very different features in the two stages. We also found it in mild as well as in virulent form. In fact, in some cases very few animals died, whereas in others there were no recoveries. The majority of the outbreaks were of a very severe type. So we have been able to observe the disease in all its aspects.

We visited various portions of the state and took along all the best apparatus and appliances for our work. Material collected in this way was brought back to the laboratory and thoroughly examined. The germs which we found were cultivated and grown on suitable materials, and were tested upon mice, rabbits and hogs. We obtained them from the spleen, liver, blood, and intestinal ulcers. In some cases we failed to get any germs at all, and although we obtained in other cases several different kinds, we have no reason to believe that more than one of them is concerned in the disease.

The only other organism deserving special attention is one which we regard as identical with the bacillus of hog cholera of Salmon, in the report quoted above, which the reader is referred to for a complete description of the same.

We have not been able to produce the disease by subcutaneous inoculations of even quite large amounts (5 c. c.) of bouillon cultures of this bacillus, but we have succeeded by feeding 300–500 c. c. bouillon cultures in milk made alkaline with carbonate of soda.—(Bulletin No. 6, New Series II, July, 1888, South Carolina Experiment Stations.)

At the meeting of the United States Veterinary Medical Association held at Washington, September, 1891, Dr. A. W. Clement made some remarks which bear on the report of that "Board of Inquiry," because "It has been my (C.'s) opportunity for the last three or four years of doing some work in that line myself, in association with Professor Welch, of Johns Hopkins University. I might say, in parenthesis, that my work in this line has nothing to do with my position as Government Inspector. As to what connection the organism (Salmon's swine plague germ) has with the lesions described in the reports of the Bureau, is a question on which we all might not agree. Nevertheless the swine plague organism does cause trouble. The trouble in hogs is, as a rule, in our experience, one of mixed infection. We have not had the opportunity of seeing an outbreak of swine plague (Salmon's) pure and simple. We have found that it is very hard to say when swine plague (Salmon's) is present that hog cholera (Salmon's) is absent, from the fact that swine plague kills (What, rabbits or hogs? B.) in a few hours, while hog cholera requires some days. If, then, an animal be killed and presents lesions in the intestines as are generally supposed to be characteristic of hog cholera, the statement must be very carefully considered before it is made, that hog cholera is not present. We were thrown off our track during the earlier part of our investigations. We found afterwards that hog cholera did exist in these animals that we thought had swine plague (Salmon's) pure and simple. I would simply

say in a general way that from our investigations we have found Dr. Billings is right in certain other matters.”—(*Journal of Comparative Medicine*, vol. XII, p. 549.)

In order to show how low these Department investigators have fallen in political degradation, and how they have begged for the patronage and support of the leading editors of the agricultural press of this country, I hereby append one of several letters of the same nature, which have been sent me, and with it the answer which the writer received. It may open the eyes of my European confreres to “ways that are strange and things peculiar” in that Heathen Chinees, the political investigator, in the United States.

“UNITED STATES DEPARTMENT OF AGRICULTURE,
“BUREAU OF ANIMAL INDUSTRY,

“WASHINGTON, D. C., April 9, 1890.

“*Henry Wallace, Esq., Editor of the Homestead, Des Moines, Iowa.*

“DEAR SIR: For some weeks I have been receiving a copy of the *Homestead* marked ‘complimentary’ for which I wish to return thanks, presuming that it comes from you. I have not been a reader of your paper before because it did not come to my desk, and, on account of the many duties devolving upon me, I seldom get time to consult the periodicals on file in our library. Mr. Hill, who has charge of the editorial division of the department, has frequently spoken to me about you, generally in connection with the attitude which your paper has assumed toward the Bureau of Animal Industry, when discussing the subject of inoculation as a means of preventing hog cholera, and he has always referred to you in such complimentary terms that I venture to address you this personal letter in the hope that any misunderstanding which you may have of our position may be explained, and that we may, if possible, obtain your good will even though our opinions may continue to differ.

“The Bureau of Animal Industry is endeavoring to work for the best interests of the stock owners of the United States, and we are earnestly striving to furnish information which shall be as nearly as possible impartial and reliable. I take it for granted that the *Homestead* is trying to treat its readers in the same manner. We therefore meet on common ground; we are working in the same field, and if we differ in our opinions on certain questions, is that any reason why one should publicly refer to the other in such disrespectful terms as to tend to destroy his influence and usefulness in the work in which we are both interested? Is it possible that the farmers’ cause can be promoted by

those who are in the field wasting their energies in destroying one another? I ask these questions frankly because I believe you are a reasonable man, broad enough and liberal enough not to be offended by them. Bear with me, if you please, while I make a few remarks inspired by the short article on page three of the *Homestead* of April 4. That I was not prejudiced against inoculation is shown by the fact that I was the first in the country to test it, and that before Dr. Billings began his investigations in Nebraska, I had worked out the method of cultivating the virus, the proper dose to use for inoculation, and the effect following this operation. And in the report of this Bureau for 1886, pages 60 to 70, the details of the experiments are given so fully that any one who can make a culture of germs can repeat the experiment for himself. Against my own hopes I was compelled to decide that inoculation was not a satisfactory method of preventing hog cholera. No one could be more anxious than I was to offer some solution of the hog cholera problem to our farmers, and if I yielded my opinion to the inexorable logic of the facts, should I be censured for it? Admit, if you please, that I was wrong in my conclusion, would even that be sufficient to justify the language which some of the agricultural journals delight to hurl at me and at this Bureau on account of the stand which I have taken?

"The experiments which I have made have been planned to bring out the truth and the details have been published. That they show inoculation cannot be depended upon to protect from hog cholera under the conditions of these experiments, is plain. It is possible, however, as I have freely admitted, though hardly probable, that the conditions of exposure on farms are not so severe. In that case a degree of protection which in our tests was insufficient might ward off the disease in most cases on farms. But surely this ought to be demonstrated before farmers are advised to risk their animals and spend their money in having this operation performed, and this is what has been done in any case by Dr. Billings. He has withheld the details of his tests, he has concealed his failures, and he has made claims which the facts do not justify. Take his experiment in Nebraska, where he had 1,000 hogs inoculated. Though he admitted at the time that about 400 of them afterwards died of hog cholera, he now says in his pamphlet that there was 'a reported loss of only eleven out of the whole number.' Can you recommend inoculation from that experiment, in which 40 per cent of the hogs died of hog cholera? If not, where is the evidence which favors it?

"Take the experiment in the article in the *Homestead*, to which I

have already referred, where you say 'this is the kind of proof required.' Certainly you could not have scanned the details of that experiment very closely, because there was none of the conditions observed which would give reliable results. Consider simply the two inoculated hogs. I have it on good authority that they were the survivors of a lot of hogs which had been affected with cholera. Is it not presumable that in such a case, having resisted one outbreak, they might be expected to resist another without inoculation? In other words, when a herd of swine is affected the most susceptible animals die and those which live have more than an average power of resistance.

"The proper way to make an experiment is to take a lot of twenty-five or fifty hogs which have not been exposed to the disease, inoculate half of them, then expose all in exactly the same manner to the contagion; keep all under the same conditions and note how much better the inoculated lot withstands the disease than the others. By making the kind of experiments which Dr. Billings reports one can prove anything, especially if he conceals his failures.

"I have written much more than I intended to, but I trust you will not be bored by it. The subject is an important one, and I feel sure that neither the *Homestead* nor any other paper can afford to either intentionally or unintentionally mislead its readers in regard to it. In my report on inoculation, of which a copy is mailed you, I have endeavored to place before the people the unvarnished facts, according to the evidence now at hand, and, before the conclusions therein expressed can be changed, there must be additional experiments made which are properly conducted, and which yield different results. I should be pleased to see some of the experiment stations take the matter up, and would assist them in so doing in any way in my power.

"With the hope that this letter will not reach you on your 'busy day,' I am, very respectfully,
D. E. SALMON."

"DES MOINES, IOWA, May 6, 1890.

"*Prof. D. E. Salmon, Bureau of Animal Industry, Washington, D. C.*

"DEAR SIR: Your favor of April 9 came to the office while I was absent on a five weeks' business trip, and I take the first moment of leisure after my return to reply.

"Allow me to thank you for the spirit and tone in which your letter is written, and to express the conviction that there is no need for men who differ honestly on certain measures of public policy to treat each other any other way than as gentlemen.

"I may as well say to you frankly that the practical results of the investigations of the Bureau of Animal Industry with reference to hog

cholera have not warranted any great hopes on the part of the swine growers of America. Possibly it is no fault of the Bureau. Nevertheless, it accounts for the attitude which many of the farmers of the west sustain toward it and the favor with which they are inclined to receive the views and promises of Dr. Billings. It must be conceded that Dr. Billings has, to a great extent, the confidence of the leading agriculturists of the west, and, what is more remarkable, the confidence of the leading stockmen of Nebraska. I attended their meeting in Lincoln in February, and was more than surprised when they elected him, though not a resident of the state, president of the Nebraska Stock Breeders' Association for the coming year.

"As I understand it, whether Billings' theory and practice of inoculation is correct or not must be determined by the facts. When the Bureau of Animal Industry took the position that there was no remedy for the hog cholera, and the government failed to take measures for stamping out the disease, as they did the pleuro-pneumonia in cattle, it is not surprising that the farmers should turn for relief to Dr. Billings, who gave assurance that under certain conditions he could prevent the disease, and proposed to let the practical results of his work determine the correctness of his theory. The bitter warfare that has been made on Dr. Billings, and which is believed by his friends to come largely from the Bureau of Animal Industry, only intensifies the popular feeling on his behalf, and it would seem to me that if the Bureau of Animal Industry, through its agents or employees, has been at the bottom of, or accessory to, this warfare, the best thing for all concerned is that it should cease. If Billings is either a fraud or an unbalanced enthusiast, time will very soon tell the story. If he is not, but has discovered a method by which, under certain circumstances and conditions, hog cholera can be prevented, then he is entitled to the credit.

"Some of the statements you make differ from my understanding of the facts. It is conceded, I believe, by Dr. Billings' friends, that 400 hogs inoculated were actually diseased, died of cholera, but it is not conceded that these 400 are part of the thousand to which you refer. I might mention other discrepancies, but have not leisure at this time.

"I confess to you that the report of the special commission did more to prejudice me against the Bureau of Animal Industry than any other one thing. You will pardon me if I say that it seemed to be a white-washing affair, sedulously, to all appearances as seen by an outsider, concealing facts and failing to make investigations which it was appointed to make.

"I have not, since my return, had time to go into the controversy between you and Dr. Billings fully. I stand ready to give the Bureau of Animal Industry credit for all the good work it actually performs, and to insist, as I shall do in one of the coming issues of the *Homestead*, on the enlargement of its powers with reference to the dealing with pleuro-pneumonia in New York. I also stand ready to give Dr. Billings credit for any results which he may accomplish, the object in both cases being to secure as far as possible the financial well being of the constituents of the *Homestead*.

"Very truly, HENRY WALLACE, *Editor Homestead*."

Now I honestly ask, what is the value of scientific assertions of, so called, investigators of the character represented by the foregoing quotations? I ask this because, as will be at once shown, assertions of the same nature have been made in the Report of the Department of Agriculture, and elsewhere, by the same individuals, in relation to our discovery of the bacillus *Southern Cattle Plague*.

They absolutely deny its existence. In order that there may be no misunderstanding in the matter, the literature, in so far as it pertains to the specific cause of the disease, will be placed before the reader. As has thus far been the case in connection with all the germ discoveries of the investigators of the Agricultural Department, the inciting movement leading to the so called discovery has invariably been borrowed from European investigators.

1st. We have the "micrococcus" of Pasteur's "Rouget" as the cause of our swine plague from 1880 to 1885.

2d. Schuetz work on a swine disease in Europe leads to the false description of the germ of swine plague in 1885 (Report of) and then the fortunate discovery of an organism closely resembling that of Schuetz misnamed "Schweine-Seuche," by the Department investigators, leads to the assertion of a second "widespread epidemic disease among the hogs of this country,"—a second swine plague which, as such, does not exist.

3d. Almost identically the same thing has happened in connection with the Southern Cattle Plague and its etiology. The work of Laveran and others had shown that certain *Protozoa* were in some way related to fever and ague and similar organisms, as indicated by Koch, had been found in the blood of birds and other animals living in, especially malarial districts. It is true that their etiological relation to any specific disease is a mere hypothesis, but the discovery came in very handy, and as some kind of an object, which colors and that is all there is to it, but which is in all probability some sort of coagulated necrosis of

the plasma of the red-blood cells, happened to be found in the blood of southern cattle. To see such a thing was all that was necessary. It was there in some condition or another. It mattered not whether it could be cultivated, whether it could be isolated and inoculated. Something was there and it was all that was necessary to use as a political militant to offset and disqualify the Nebraska investigations. Foreigners cannot see that unless the Agricultural Department at Washington can fool the people and congress and make them think that its investigators are doing all the work, and the only correct work, that congress will not support that institution, *because its power as a spoke in the political machine would be void*. That is the meat in the cocoanut in this dispute, so far as that department is concerned.

The first publication of this kind emanating from the Agricultural Department, was an article in the "Medical News"—Phil., of Dec. 21, 1889, entitled "*Preliminary Observations on (not 'a') the Micro-organism of Texas Fever,*" by Theobald Smith.

We need not notice this further, as the assertion is entirely without proof.

ROBERT KOCH HELPS THE DEPARTMENT OUT.

At the meeting of the International Medical Congress, Berlin, Monday, August 4, 1890, Koch said:

"In numerous places, and, indeed, in such in which it was least expected, bacteriological investigation has left us completely stranded, especially in the investigation of a number of infectious diseases, which, on account of their manifest infectiousness, appeared to offer the most favorable conditions for investigation. This has reference, beyond all other disease, to the entire group of infectious exanthematous diseases, measles, scarlet, variola, and spotted typhus. In no one of these diseases have we been enabled to establish the smallest evidence as to the character of their specific cause. Even in vaccina, which is at the command of every investigator at any time, all our efforts to discover the specific organism thereof have been in vain. The same is true of rabies.

"We also know nothing of the specific inficiens of the influenza, whooping cough; the trachoma, yellow fever, the rinderpest, and contagious pleuro-pneumonia. I am inclined to the opinion that, in the diseases named, the specific excitor is not to be sought in bacteria, but in disease-producing organisms which belong to entirely different groups of micro-organisms. This conclusion is all the more justified for the reason that lately have been discovered peculiar parasites in the blood of some species of animals, but also in that of men in mala-

rial diseases, which belong in the lowest classes of the animal kingdom, the protozoa. Beyond the simple demonstration of these remarkable and very important parasites we have not gone."

"Ueber Bacteriologische Forschung," *Deutsche Med. Wochenschrift*, 1890, p. 757.

From the very moment that I read these words of the father of modern bacteriological technique, and certainly one of the most noted and painstaking investigators the world has ever produced, I could not help, and cannot yet restrain expressing the utmost astonishment that Robert Koch could have allowed himself to make such an assertion, or come to any such conclusion. Both are absolutely contradicted by every iota of pathogenic experience in reference to the diseases mentioned by Robert Koch. What correspondence is there in the genesis of the influenza, whooping cough, scarlet, measles, variola, spotted typhus, the rinderpest and contagious pleuro-pneumonia, with a malarial disease? They are, one and all, endogenous, obligatory parasitic diseases, while all malarial diseases are paradigmatic of exogenous, facultative parasitic diseases. Where is the danger of transmission in any natural way of specific malarial diseases from diseased to healthy individuals? The diseased individual is as harmless to others as a cooing dove. Whoever heard of fever and ague being transmitted by cohabitation? Whoever heard of house-quarantine, of burning all the clothes, or scalding or severe germicidal measures, of shutting up school houses, as in the case of the diseases named in man; or of calling out the military, or absolute slaughter as in rinderpest and contagious pleuro-pneumonia in cattle, being resorted to in any disease of malarial character? These diseases are specifically "contagious" in the ancient hygienic and common sense view, equally scientific, of the origin of diseases. *Without the presence of a diseased individual, or something directly from such, no danger.* Koch's *a priori* hypothesis is no more fortunate as regards the yellow fever, and diseases of that class, diseases of local origin, exogenous facultative parasitic, but in which the diseased individual can transport the specific inficiens, if traveling or moved when ill, to other localities and infect them, whereby persons in those localities, if they offer favorable conditions to the continued development of the germ and the retention of its virulence, may also become infected and acquire a disease foreign to their clime. *Hence quarantine regulations in these diseases also.* So far as the experience of the centuries go, and they cannot be passed over as worthless, no malarial disease has ever been, transportable in any such manner, and no such restrictions on the movements of persons from

malarial districts have ever been advocated. Even though the specific inficiens of fever and ague has been generally admitted to have been discovered by Laveran and others, still all attempts to transmit the disease to healthy organisms with it have failed. I admit this does not amount to anything until the transmission to human beings has been tried. On the other hand it is now pretty certain that the specific-causal agent in three of the diseases named by Koch, influenza, spotted typhus, and measles, has been discovered, and there are rumors in the air of the discovery of a similar agent in variola and vaccina, one of which, that of vaccina, I have some knowledge of and think to be reliable, by my friend Dr. S. C. Martin, of Boston.

It has always seemed to me that a person of Koch's great experience in bacteriological investigation, and one who certainly knows, as well as any man, of the great difficulty, at times, of the investigator's supplying artificial media and conditions exactly conformable to those offered by the animal organism to the life of specific inficientia, would have at once seen that in this fact was to be sought the cause of the failures he has mentioned, and that any such *a priori* hypothesis as he then made regarding their origin was absolutely without a particle of evidence in its favor. Even blood serum, when drawn from the body, most carefully and successfully separated and sterile, though uncoagulated, is no longer necessarily a natural nutritive medium, though the changes thus produced in it are not of so radical a nature as to interfere with the extra-organismal development of many germs, while they are to others, and that is why we have failed to cultivate them and prove their specific infectious disease-producing qualities.

THE RELATION OF TICKS TO TEXAS CATTLE FEVER.

Sixth and Seventh Reports Bureau of Animal Industry, 1889, 1890. Revised 1891.

While the investigations into the nature of this disease were going on, other equally important work was being carried on at the Experiment Station on the external characters of the disease.

It is well known to those who have come in contact with southern cattle in summer, that they are infested with the so-called cattle tick, a pest belonging to the class Arachnida and to the family Ixodidae. These ticks are carried north with cattle during the warm season of the year. When fully matured they drop off from the southern animals, lay their eggs on the ground, and perish. The young ticks are hatched within fifteen to thirty days after the eggs are laid, and at once get upon the cattle, where they become mature within twenty to thirty

days, to again drop off, lay their eggs, and die. This process goes on continually until the cold weather comes.

At various times and in different parts of the country it has been suggested that the ticks were the cause of Texas fever in northern cattle. This inference was undoubtedly suggested by the fact that nearly all cattle that die of Texas fever are observed to have these ticks, of various sizes, attached to the skin. Moreover, the disease only makes its appearance after the young ticks have attached themselves to cattle. Though this was purely a *post hoc propter hoc* inference, it was, nevertheless, true, as the experiments to be recorded will amply prove.

During the summer of 1889, Dr. F. L. Kilborne, in arranging the various inclosures at the Experiment Station for the exposure of native cattle to the infection of Texas fever, conceived the happy idea of testing this popular theory of the relation of ticks to the disease. This he did by placing southern (North Carolina) cattle with native cattle, in the same enclosure, and picking the ticks from the southern stock as soon as they had grown large enough to be detected on the skin. This prevented any ticks from maturing and infecting the pasture with the eggs, and hence prevented any ticks from infecting native cattle subsequently. At the same time, in another enclosure, the ticks were left on the southern cattle. The natives in the latter field died of Texas fever, those in the former did not show any signs of the disease.

Another experiment was made in September in the same manner by preparing three fields, one with southern cattle and ticks, a second with southern cattle, from which the ticks were removed, and a third, over which only adult ticks had been scattered. The result was equally positive. In the first field no natives died, but careful examination of the blood by the writer showed Texas fever in an unmistakable manner. In the "tick" field one animal died of Texas fever, and the examination of the blood showed that most other natives in the field were sick. In the third field, containing southern cattle without ticks, no disease could be detected.

These two tests pointed directly to ticks as being in some way the cause of Texas fever. At the same time it was thought best to confirm these results by further experiments during the present year before other agencies could be eliminated. The immediate inference was that the ticks infect the pastures, and that in some unexplained manner the infection finds its way into the body of susceptible cattle. The preliminary conclusions deducible from the work of 1888 and 1889 can be formulated as follows:

(1.) Texas fever is a disease not caused by bacteria. Its nature cannot be understood by supposing a simple transfer of bacteria from southern cattle to pastures, and from pastures to northern cattle.

(2.) The cause is very probably a protozoon, with a more complex life history, living for a time within the red corpuscles of infected animals.

(3.) Southern cattle without ticks cannot infect a pasture.

(4.) Ticks alone, scattered on a pasture, will produce the disease.

The work of 1890 was planned to confirm or refute these preliminary conclusions and to furnish additional information.

The fields were arranged as before. One contained North Carolina cattle with ticks, a second Texas cattle with ticks, a third North Carolina cattle without ticks, a fourth ticks only, and a fifth, soil from the pastures of infected North Carolina farms. Other fields were also laid out to test questions which need not engage our attention in this brief survey.

The results confirmed those of last year. The first animal to die was in the "tick" field containing no southern cattle. No disease appeared in the soil field. *Unfortunately, owing to the limited space of ground at our disposal, and its barren, rolling character, ticks or eggs were washed, during the very heavy rains of the summer, from the tick field into the field containing northern cattle without ticks, although a wide lane intervened. The natives in this field thereupon all died with Texas fever.* At the autopsy of these cases ticks were found attached to their skin in abundance.

The disease caused by Texas cattle could not be distinguished in character from that which was produced by North Carolina cattle. These results similarly pointed to ticks as the cause. The precise manner in which they caused the disease was by no means clear, however. The theory which seemed for a time most acceptable was that the adult ticks, as they dropped off, infected the pastures with germs which they had taken in with the blood of southern cattle, and that the germs were introduced into the body of northern cattle with the food. At the same time no parasite could be detected in the blood of southern cattle examined at various times, on which fact I would lay no great stress, however. Of more importance is the peculiarity which is exhibited by this disease in its period of incubation, as it may be provisionally denominated, and which is opposed to this theory. Thus, when native and southern cattle are placed on the same pasture at the same time, it will take from forty to sixty days for the disease to appear. After the disease has once shown itself, fresh animals placed

on the same pasture may die, according to our experience, within thirteen days after the beginning of the exposure. We might say that the virus has "to ripen" on the pasture, which takes nearly two months, depending on meteorological conditions. When once "ripened" this virus does its deadly work within two or three weeks. This explanation, however, would be merely formulating our ignorance concerning the true nature of the infectious principle.

To the writer there seemed to be but one inference drawn from the facts, and that is that the presence of young ticks is in some way directly associated with the appearance of the disease. It requires from forty to sixty days for the matured ticks to drop from the southern cattle and the eggs laid by them to develop into young ticks. After that period young ticks are present on the pastures until they are destroyed by the cold, or until the cold interferes with the development of the embryo in the egg. In other words, the period of incubation of the disease is explained without any difficulty by the life history of the tick.

The question was solved, experimentally, in the following manner : Eggs laid by ticks sent from North Carolina were placed on dried leaves in a dish partly filled with moist soil, and kept in the laboratory until the young emerged from the egg. The period of incubation depends entirely upon the relative amount of heat, and has varied from fifteen days in midsummer to forty days in November, when the rooms of the laboratory became cold at night (50 to 60 degrees F.). These ticks were placed on four different animals of different ages, kept away from any infected enclosures. Two were placed in stalls, one of them on an adjoining farm, and two were allowed to stay in a patch of woodland with healthy cattle. Of these four, two died of Texas fever, as determined by careful *post-mortem* examination. One of them was in the stall away from the station, the other in the patch of woodland. The other two became very ill; one of them never recovered, but had to be killed later on; the other recovered. In all of them the germs were observed in the blood. The disease possessed the same characters as those observed in cattle in the infected pastures during the summer. There was an elevation of temperature from nine to twelve days after the young ticks were placed on the animals, going as high as 107 degrees F. in one animal. Accompanying the fever a gradual reduction in the number of blood corpuscles was observed. In order to show more conclusively the truth of the statements made, a few brief notes from one of the experimental cases is appended:

No. 144—Cow about eight years old, purchased September 16 from a neighboring

farm, and placed among a number of healthy reserve cattle in a piece of woodland at some distance from any infected field.

September 17.—A considerable number of young ticks, hatched in the laboratory from the eggs laid by ticks from North Carolina, placed on this animal.

September 18—Temperature 101.2° F., 6.3 millions red corpuscles in blood; normal.

September 24.—Another lot of recently hatched ticks placed on the animal.

September 27.—A. M., temperature 104° F.

September 29—10.45 A. M., temperature 106.2° F., pulse 54, respiration 27, 4.93 millions corpuscles in blood. Ticks abundant on body, especially on inside of thighs. Still quite small.

September 30.—P. M., 107°.

October 1.—P. M., 106.3°.

October 2.—P. M., 104°.

October 3.—Found dead this morning. Seen alive at 6 P. M. yesterday. A large number of ticks on animal, just through second molt. None of them large as yet.

Lungs only partly collapsed; trachea and bronchi filled with foam. Ecchymoses under epicardium of both ventricles of heart, and under endocardium of left ventricle.

Spleen very large, blackish, soft, weighs 4 1-16 lbs. (Normal weight about 2 lbs.)

Liver weight about 12 lbs.; enlarged, yellowish on section. Complete injection of the intra-lobular bile capillaries. Extensive fatty degeneration of liver tissue. Occasional groups of hæmatoidin crystals.

Bile dark, scarcely flows. Density due to large quantity of yellowish flakes and mucus.

Kidneys deeply congested; tubules contain much yellowish pigment. Urine in bladder of a deep, port wine color, barely translucent in small test-tube; alkaline; specific gravity 1015; no sediment; albuminous precipitate very abundant (.6 per cent according to Eabach). Heavy flocculent precipitate when acetic acid added (hæmoglobin).

In preparations of blood from the heart, of liver, spleen, and kidneys, a small number of corpuscles contain parasites, in contracted state, from 1.5 to 2. micro-millimetres in diameter. In the blood, spleen, liver, and kidney preparations, a moderate number of large bacilli of post-mortem growth. (These bacilli are invariably present when the animal dies early in the night and is not examined until next day. They are never found in animals killed in a dying condition. They occur in other diseases under similiar conditions.)

These brief notes demonstrate that Texas fever can be produced by placing young ticks on cattle, and that the disease can not be due to any abstraction of blood, for the ticks were still quite small, and had scarcely begun to draw blood on a large scale. Moreover, the corpuscles perished *in the body*, as is shown by the coloring matter in the urine, by the thick bile, and the presence of pigment in the liver and kidneys. No disease appeared among the other cattle in the same enclosure.

While the nature of the Texas fever is by no means made clear as yet, we are able to affirm that ticks can produce it. Whether the disease can be transmitted by any other agency must be decided by future investigations. Meanwhile the evidence accumulated thus far seems to favor very strongly the dictum—*no ticks, no Texas fever*.

So particularly attractive and so valuable a scientific contribution did this last appear that it received a lengthy editorial notice in the London *Lancet* of April 2, 1892, which was copied into the *Veterinary Journal*, of London, for May, 1892, from which the following is taken :

“IXODES AS A CAUSE OF INFECTION IN CATTLE.

“For many years a mysterious disease has affected cattle in that part of the American continent which borders on the Mexican gulf, and is known in the United States as the splenic, Texas, or Spanish fever. It has been observed wherever and whenever bovines from the states on that gulf have been driven northward, and more particularly through the summer months, and is looked upon with the greatest apprehension by cattle breeders. In 1868, for instance, the destruction it wrought among the northern herds with which the traveling Texan droves came in contact, directly or indirectly, and the obscurity that prevailed with regard to the nature of the disease, caused a serious panic, and nearly led to the total suppression of the traffic in cattle between the gulf and states (which are almost entirely cattle-rearing countries) and the northern states. At that time Prof. Gamgee was in America, and the United States government availed itself of his services in making an investigation of the malady. The report of the commission was published in 1871, and did not throw much light either on the nature of this Texas fever, as it is now generally designated, or its etiology ; indeed, the latter remained in as great obscurity as ever. The tendency was to regard it as a kind of anthrax or splenic fever, due to the animals becoming affected by something in the soil or water, their systems becoming charged with poisonous principles that accumulated in the bodies of acclimatized cattle, which enjoyed immunity from the disorder. It was ascertained that those southern cattle might for some weeks, and probably for as long as three months they would ‘continue to excrete the deleterious principles which poison the cattle of the states through which the herds are driven on their way north or west.’ The report also stated that all breeds of cattle in states north of those of the gulf coast, without regard to age or sex, if they fed on grass contaminated by southern droves, were attacked with fever ; that it occurred chiefly during the hot months of summer and autumn, and that infected pastures were rendered free from danger by frost, and remained so while fresh droves of Texan or Florida cattle were driven again over them.

“This apparent freedom of southern cattle from disease, and their capability, notwithstanding, of disseminating such a fatal infection

among northern bovines, was an altogether unusual feature in epizootology and could not be accounted for. It is true that the redoubtable cattle plague or rinderpest does not so severely affect the cattle of southern Russia as it does those in western countries; yet it kills a good percentage of them, and greatly impairs the condition of those which do not succumb. But here were animals in evidently excellent health, carrying destruction everywhere they went. The principal symptoms in those infected are somewhat rapid rise of temperature, from 101, 102, 106, and 107 degrees, at which it remains from four to fifteen days in fatal cases; this condition is soon accompanied by intense debility, that persists until death takes place. A few days before the fatal termination the urine becomes more or less deeply tinged with hæmoglobin, and the destruction of red blood corpuscles, causing this altered tint of the urine, can be demonstrated by examining the blood obtained from an incision in the skin. In acute cases, a day previous to death, the number of these corpuscles had been found reduced to about one million in a cubic centimetre of blood, the normal number being something like five million and a half. In one case, which recovered, the corpuscles fell to nearly one-half their ordinary number, and a week later they had increased to three millions. This enormous decrease in the red blood corpuscles gives the blood an exceedingly thin and watery appearance; after death it coagulates rapidly and forms clots of unusual size.

"The lesions noted in making a necropsy are chiefly the following: The spleen is very much enlarged, and when incised the tense capsule retracts and exposes a dark red and more or less disorganized pulp, that sometimes pours out as a semi-fluid mass. This engorgement is due chiefly to the presence of an immense number of red corpuscles. The liver is much affected; it is yellowish-brown in color; the tissue is deeply bile-stained, and the smaller canaliculi are often plugged with inspissated bile, that fluid in the gall-bladder being so thick that it scarcely flows, owing to the presence of solids in the form of minute yellow flakes. The kidneys are tinged with hæmoglobin, the connective tissue around them being infiltrated with reddish serum. The other organs are often more or less stained in the same manner, but seldom exhibit any other character.

"The heavy losses caused every year by this peculiar fever have compelled the United States government to take special notice of it, and with the view of thoroughly investigating its pathology, experimental stations have been established at points where it could be best observed. The result of this measure has been very satisfactory. These investi-

gations began in 1889, and attention was more especially directed to the blood. No bacteria were discovered, but there were found, instead, peculiar bodies lodged in the red corpuscles. In the fresh splenic pulp these appeared as round or oval, almost colorless spots, from one-half to nearly two micro-millimetres, from $\frac{1}{50000}$ to $\frac{1}{1200}$ inch in diameter, on the disc of the red corpuscles, and always somewhat eccentrically placed. Sometimes there was only one, but quite commonly there were two and very rarely three or four in the same corpuscle. In organs kept in the cold for nearly two weeks, these bodies were still visible, though faintly so, owing to the diffusion of the hæmoglobin around and perhaps into them. They stained with analine dyes as readily as nuclei and bacteria, and retained the stain as tenaciously. They have appeared like deeply stained cocci, about one-half to one micro-millimetre in diameter, situated within the unstained circle of the corpuscle. Besides the spherical forms, ovoid bodies are not uncommon, and these usually occurred in pairs within the red corpuscle; while a still rarer pear-shaped form was met with in stained preparations of the blood; this being rounded at one pole and constricted or filamentous at the other. These almost invariably occurred in pairs, which occupied one corpuscle, and were considered as the result of division of the single body. Another abnormal form was found in the blood. When dried cover-glass preparations were stained with Löffler's alkaline methylene blue, and few corpuscles appeared as if their surface had been dusted over with minute specks of coloring matter; but whether these were due to the anæmia or whether they belonged to the cycle of the micro-organism, could not be determined. Very few of these bodies were found in the circulating blood, though in one instance a number were detected in this fluid the day before death. Blood from the right ventricle of the heart contained them in enormous numbers, in pairs, within the red corpuscles. They seemed to be filtered out of the spleen and liver, for they might be numerous in one or two of these organs, and rare in blood from the right heart. They appeared to be more abundant in the spleen and liver. In perhaps half the number of cases examined, they were so few in the spleen that they easily escaped the attention of an observer searching for bacteria. In one case they were so scarce that a number of fields had to be scanned before any were detected, and here the spleen was completely disintegrated, while the urine, which contained much hæmoglobin several days before death, was almost normal at the necropsy, as if the animal had overcome the parasites, but perished from the havoc caused by them.

"It had long been suspected by the cattle owners that the appear-

ance of this fever in northern cattle was in some way connected with the ticks (*Ixodes Americanus* Lat.; *I. bovis* Riley) distributed by southern cattle, though this notion was generally scouted by scientific men. However, as every other possible cause of the scourge had evaded detection, it seemed worthy of investigation, and the result now is that indisputable evidence has been obtained that the fever is produced by these ectozoa. Ticks taken from the bodies of southern cattle and placed upon pastures which could have been contaminated in no other way, so infected these grounds that susceptible bovines placed upon them contracted the malady in the same period of time, and were as seriously affected as were the susceptible cattle allowed to consort with those from Texas. Again, young ticks were hatched from the eggs of adult ticks picked from southern cattle, placed on the skins of northern animals, and induced the disease.

"There are, consequently, two factors in the production of Texas fever, ixodes and the protozoal micro-organism which is located in and destroys the red corpuscles of the blood in the affected animals. Wherever the insect obtains the protozoon is not yet known, but that the latter can be transmitted from one generation of ticks to another, through the ova, has been demonstrated. It is important to know how many generations of ticks these germs can be communicated to without losing their virulence, and whether there is any other means by which they gain access to the blood of cattle than by the punctures made by these vermin. There are evidently ticks which do not bear the micro-organism, as cattle which are susceptible to Texas fever are often badly infected with them without showing any marked sign of bad health. On the other hand, there may be means by which the protozoon gains access to the blood of cattle independently of the agency of ticks; but it appears from the investigation just reported, that in the great majority of cases cattle are infected by means of the skin parasites. The adult ticks drop from the southern cattle and deposit their ova upon the pastures; these ova are hatched and the young ticks get on susceptible beasts and engender the disease in them. The young ticks are hatched within from fifteen to thirty days after the eggs are laid, and at once get on the cattle, where they become mature in from twenty to thirty days; they then fall off, lay their eggs and die. This process goes on continually until the cold weather sets in.

"The experiments by which these interesting and important results were arrived at, were numerous and conclusive as to the part the ectozoa play in the production of the malady, so that it might be said

that, no ticks, no Texas fever. Is it not possible that the Tse-tse fly of central and south Africa causes the death of certain mammalia by acting as the intermediate bearer of a noxious micro-organism, as the mosquito does with regard to the *filaria sanguinis hominis*? In no other way, it would be seen, can its deadly action be accounted for.”
— *The Lancet*.

A SUMMARY OF THE STATEMENTS OF THE GOVERNMENT INVESTIGATORS.

“It is well known that southern cattle, in summer, are infected with the so-called cattle-tick. The ticks are carried north with the cattle during the warm season of the year. When mature they drop off the southern cattle, lay their eggs on the ground and perish. The young ticks are hatched fifteen or thirty days after the eggs are laid, become mature within twenty or thirty days, to again drop off the cattle, lay their eggs, and die. This continues until cold weather comes. The disease only makes its appearance (in northern cattle) after the young ticks have attached themselves to them.

“The idea has often prevailed that the ticks were in some way connected with the disease in northern cattle. Experiments were made to test this theory.

“Southern cattle were placed in the same enclosure with native (northern) cattle, and the ticks picked off the southern cattle as soon as they were large enough to be detected on the skin. This prevented any ticks from maturing and infecting the pastures with eggs, and hence prevented any ticks from infecting native cattle subsequently. At the same time, in another enclosure, the ticks were left on the southern cattle. The natives in the latter field died of Texas fever, those in the former showed no signs of disease.

“Fields were prepared. 1. With southern cattle with ticks. 2. With southern cattle from which the ticks had been removed. 3. One over which the removed ticks were alone scattered. In field number one two natives died. In the tick field one animal died and others said to have been ill. In the third field, where no ticks were on the animals, none died. These results lead to the following conclusions:

“1. *Texas fever is not caused by bacteria.* Its nature cannot be understood by supposing a simple transfer of bacteria from southern cattle to pastures and from pastures to northern cattle.

“2. The cause is very probably a protozoon with a more complex life history, living for a time within the red corpuscles of infested animals.

"3. Southern cattle without ticks cannot infect a pasture.

"4. Ticks alone scattered on a pasture will produce the disease."

How the Disease is Transferred from Cattle to Cattle by Ticks.

"There seemed but one inference to be drawn from the facts, that is, *that the young ticks are in some way directly associated with the appearance of the disease.* It requires from forty to sixty days for the matured ticks to drop from the southern cattle and the eggs laid by them to develop into young ticks. The period of the incubation of the disease is explained without any difficulty by the life history of the tick.

"*The Question was Solved, Experimentally, in the Following Manner:*—Eggs laid by ticks were artificially hatched. The young ticks were placed on four different animals, which were kept away from infected enclosures. Of these four, two died of Texas fever, as determined by a careful *post-mortem* examination. The other two became very ill. In all of them the germs were observed in the blood."

On its face the above statement of apparent facts certainly looks very conclusive, but the careful reader, especially if one previously acquainted with the character of other so-called investigations from the same source, will at once notice certain serious discrepancies and certain striking loop-holes in the chain of evidence, which a due regard for scientific exactness cannot well overlook, and they should not be overlooked. They are especially called to the attention of the reviewer of this work in the *Lancet* and the editor of the *Veterinary Journal*, as well as to that of scientific investigators in general.

They are:

1st. The contradiction between doubt and certainty. In the second conclusion it says, "the cause is very *probably* a protozoon, and the tick is the transmitter of the protozoon." In another place there was no probability about it, for "the question was solved experimentally" in the manner quoted, by placing artificially hatched ticks (from the ova) on northern cattle.

If it was solved it could no longer be problematical. In conclusion "1," it says: "Texas fever is a disease not caused by bacteria." In another place it says: "In one of them (the cattle) the germs were observed in the blood." Perhaps the difference between "germs and protozoa" is immaterial, and I am but making a play on words. The whole story minutely betrays uncertainty of his position on the part of the writer. In no sense have we a particle of true scientific evidence, such as we all have a right to expect of one another in such matters.

*We have no illustration of any organism whatever.** Not a plate culture was made from the blood or tissues of any of these animals, nor even do we have a particle of evidence that bouillon or any other cultures were attempted.

No mention is made of the inoculation of any kind of small animals (a favorite procedure with these investigators when convenient) either with blood or tissues of the sick or dead cattle. (It is self-evident that cattle under such immediate control could have been killed in time to avoid any post-mortal pollutions). No mention is made of attempts at transmission with the blood or tissues of the diseased cattle, in cattle themselves, a most imperative test experiment in such cases, which should, it would seem, naturally and at once suggest itself to any investigator under the circumstances. In other words: If the protozoa are in the blood of cattle diseased with a specific disease, and the protozoa are the hypothetical cause of the disease, the best way and only way to that question would be:

1st. By plate cultures to assure one's self that no developable germ, in or on every available media, was in that blood.

2d. A similar system of isolation by the use of small animals (the zoospore might fortunately kill in that case, which would be a valuable link in the chain of evidence).

3d. This being done to prove indeed that it was the zoospore (and not something else in the tick), the inoculation of healthy northern cattle with blood known to contain the zoospore and nothing else was absolutely indicated.†

4th. As anthrax can be transmitted by flies, mosquitoes, etc., and as yellow fever is claimed to have been transmitted by the latter, a kindred disease to southern cattle plague (as I have shown in a former edition of this work), why were not the old ticks, with their soiled probosces, placed on healthy northern cattle?

* One has since been published in a book on cattle diseases, given away by the Agricultural Department.

† In the report of 1891, just out, it says: "Last year blood from diseased animals was injected directly into the jugular vein of two healthy animals; subsequent examination indicated a mild attack of Texas fever, but the result was not positive enough. This year the blood of diseased animals was injected into the jugular vein of three healthy animals, and in all a very acute form of Texas fever was produced within four or five days. One died, others recovered. There can be no doubt that the disease is inoculable from the blood of one animal to another, when the germs are transferred quickly." P. 132. Again, there is no record of plate culture, or a proper examination of the blood. It seems to me that the above must be considered very poor experimentation. If the blood causes disease, which there is no reason to doubt, there must have been some specific object in that blood, something demonstrable. Zoospore diseases have not yet been satisfactorily transmitted in that manner. As no attempts recorded of are endeavors to demonstrate any other inficiens in the blood, it must be admitted that it looks as if these experimenters were afraid to make such.

Had these things been done and fallen negative, they certainly would have been mentioned.

Again, and of equal importance: The "protozoa" are claimed to be the *causa sufficiens*, the inficiens proper, transmitted, and the tick asserted to be the mediator of transmission: the virus.

Where do the parent ticks obtain the protozoa? Naturally from the blood of the southern cattle! Then the protozoa should have been as easily demonstrated in slides from the inside, the blood of the ticks, as in the blood of cattle, but we find no mention of the examination of the ticks in this direction, nor of plate cultures made of their blood.

The protozoa in some way get into the ova and develop up with the young tick; the ova were in the possession of these men, the protozoa is claimed to be easily colored; why have we no mention of attempts then being made to demonstrate these things in the ova? Why not in the mates of the young, artificially hatched ticks, from the ova, when they were put on cattle?

With all due respect to the editors of the *Lancet* and *Veterinary Journal*, I think all unbiased and competent readers must at once admit that they have accepted, and in some measure endorsed, the "fishiest yarn" yet promulgated in the name of scientific investigation. A "yarn" absolutely without a scintilla of exact support, except the mere fact of infection, which no one denies, and a second fact, that infection may be in some way connected with ticks.

Let us analyze this tick question a little closer, for it is undoubtedly enchantingly interesting.

So far as they can be hypothecated from the statement of the Agricultural Department we have these conditions to consider:

1st. The mother ticks from Texas are, of and in themselves, harmless. (It will be remembered that a proper test to prove that they were not was neglected.)

2d. They must suck the protozoa from the blood of the diseased cattle.

3d. They drop on the ground, when mature, from the diseased cattle, lay their ova and die.

4th. The protozoa is kind enough to crawl from the intestines of the mother tick, through their walls, and penetrate the membrane of the ovum and develop up with the young tick, without injuring either ovum or embryo, then crawl into the intestines and stomach of the young tick.

5th. The young tick, with its protozoic accompaniment, crawls up the legs and onto the bodies of convenient northern cattle, and what

does it do? Contracts its abdomen and through its proboscis injects the northern cattle. (It has never been near southern cattle, according to the story.)

6th. Do the protozoa crawl out of the intestines of the mother tick, and attach themselves externally to the ova and hold on and pass out of the northern tick, and still hold on to the embryo, and are carried by it onto the cattle and then crawl off and into the blood-vessels of the cattle through their hides?

These six postulates may appear ridiculous, and I believe they are, to the reader, but in no other way possible can the "sporozoon-tick" hypothesis of the government investigators be explained, so far as I can comprehend their statements. As "sporozoa," or anything akin to them, have only been found in malarial disease, and as the essential differentiating characteristics of all malarial diseases is local infection with the absolute impossibility of transmission to other individuals, or of the inficiens to other localities by the sick individual, thus far in their history, it looks as if this story from Washington must be taken *cum grano salis giganticus*, and valued on a par with the balance of the testimony from the same source; though there is a germ of truth in the tick, as will be conclusively shown later on.

As I have taken especial pains to show, by quotations from previous announcements and by the universal testimony on those announcements, nothing milder than the Scotch verdict of "not proven" is justifiable in this case.

EXTRACTS.

Extracts from a paper read before the Twenty-seventh Annual Meeting of the United States Veterinary Medical Association, at Chicago, September 17, 1890, and published in THE JOURNAL OF COMPARATIVE MEDICINE AND VETERINARY ARCHIVES October, 1890, pp. 542-3.

"We will now turn our attention for a few minutes to the subject of Texas fever. While this disease, as you know, is allied by its characters to the bacterial fevers, it has certain marked peculiarities, which have caused it to be regarded as the most mysterious malady which remains for modern science to investigate. For four years we have been giving the disease very close attention and study, and our results have been unexpected to ourselves, and so different from those reached by other investigators, that we have not pressed our conclusions until we had taken every precaution to confirm them.

"I am prepared now to say that Texas fever is not a bacterial disease. There are no peculiar bacteria to be found in the blood, spleen, liver, or

other organs ; and as a rule the blood and tissues are free from even the common septic bacteria when examined immediately after the death of the affected animals. All the illustrations which have been published showing preparations of blood from Texas fever animals swarming with bacteria, and sections of the tissues showing the same micro-organisms, are misleading, and may be put aside as of no value to the student of this disease. The blood and tissues do not present such appearances when properly examined.

"Texas fever belongs to the malarial type of diseases. The germ resembles the malarial organisms of Laveran. It is found within the red corpuscles of the blood, and may be demonstrated in them before the death of the animal. We have not been able to cultivate it, and do not believe that it can be cultivated by any of the methods now in use by bacteriologists.

"We have studied outbreaks that were caused by cattle from the tide-water section of Virginia, from North Carolina and from Texas, and in every case we found the same micro-organism present in the blood. In some cases almost every corpuscle would be invaded. These parasites rapidly destroy the corpuscle, and the number of these soon falls to one-half, one-third, one-fourth, or even one-fifth of what is found in health. This destruction of the corpuscles, of course, accounts for the excretion of the red coloring matter in the urine.

"The possibility of the infection of pastures by ticks has also been demonstrated. For two years we have picked the ticks from southern cattle and scattered them over experimental pastures, and we have found that susceptible cattle would contract the fever from these pastures as readily as from those upon which the southern cattle themselves had been placed. It is a fact, therefore, that ticks may infect pastures without the presence of southern cattle. Whether southern cattle can infect pastures without the presence of ticks is still an open question. We had an experiment planned to settle this problem the present summer, but it failed by the unexpected appearance of the ticks upon this lot of cattle which we had supposed were protected from them.

"The main results of our Texas fever investigations may, therefore, be summed up as follows :

"1. Texas fever belongs to the malarial group of diseases.

"2. It is caused by small micro-organisms which are found in the red blood corpuscles, and which are not bacteria, but bodies of the nature of the malarial germs of Laveran.

"3. Pastures may be infested by ticks from the bodies of southern cattle."

Bold words those in the light of the investigations previously published in Nebraska.

FORMER INVESTIGATIONS FROM THE SAME SOURCE.

Any one who reads the account of the investigation of one of the subdivisions of the Agricultural Department as published in the reports lately referred to, and in the spring of 1891, would scarcely realize that the chief of that division had published quite positive testimony as to a specific germ in Texas fever many years before. One would naturally think that a spirit of common honesty, both as a public servant of a great nation, as well as in obligation to scientific investigators the world over, would have induced the chief of the Bureau of Animal Industry to have especially admitted that those investigations were erroneous. Of course it may be said that the assertion that "Texas fever is not caused by bacteria" is sufficient, but to my mind that is not enough under the circumstances. Some allusion should have been made to the previously published assertions, which I now quote merely to show that these same investigators, or the chief of them, once did assert "bacteria" to be in the tissues of Texas fever diseased cattle, and that he was in the same maze of uncertainty as to their value as at present regarding the "protozoa," at one time asserting the cause discovered, and another, not being quite so sure on the matter.

The following quotations are taken from the Report of the Department of Agriculture, 1883, on the

"ESSENTIAL NATURE OF TEXAS FEVER.

"If it had not been for our private laboratory, where our microscopical apparatus and reagents could be used to advantage, and where we had fitted up an incubator, imperfect for many purposes, it is true, but still one in which cultivation could be made very successfully, the scientific investigations for the year with this disease must of necessity have been a failure, as such investigations have so generally been in the past" (p. 33).

Now, let us be honest and unprejudiced; what do we understand the government investigator to mean by those words? Can we place any other meaning on them than that he considered his laboratory work, in the investigation of Texas fever, to have been successful, for does he not almost positively compare it in that way with all previous investigations?

"August 1, 1882, I was successful in filling and sealing a number of

tubes from the spleen of a diseased cow which had just died. August 7, one of these vacuum tubes was examined microscopically. No decomposition whatever had occurred. Two cultivation tubes were infected with some of this pulp as soon as the tubes were opened; one was found to be a pure cultivation of diplococci," the other was impure (p. 35). "Two tubes containing splenic pulp were in excellent condition. Both contained the diplococci found in a former case."

"In 1880, two inoculations were made with splenic pulp, one of which produced a very severe case of the disease, the second was milder, though accompanied with great elevation of temperature" (p. 36.) Some other experiments failed with the same material, but on "the 3d of October I obtained some foul material by killing a cow in the last stages of the disease, and in the afternoon we inoculated a cow in the side of the neck with the splenic pulp obtained in the morning. This animal died in the night of October 16. * * * In this experiment we have a very evident confirmation of my former investigations. The disease is then inoculable and the splenic pulp is the material which contains the virus. Out of five inoculations with this material, we have produced three plain cases of the disease. Inoculations were also made with the cultivated micrococci." They proved negative. "They do not show that our diplococcus is the cause of the disease, nor do they show that it is not" (pp. 37, 38).

Is it not strikingly singular, in the face of the last of the quotations, that the chief of the Bureau of Animal Industry should have previously said, in the same report, "The scientific investigations for the year with this disease must have been a failure, had it not been for our private laboratory, as such investigations have so generally been in the past"? In one place he certainly claims success and in another admits failure to prove his position.

Does it not seem natural that in resuming those investigations in 1888 or 1889, some attempts at similar inoculations should have been made by the same investigator to prove or disprove the observations of previous years?

Does not the results of those years' investigations intimate that some bacteria were present in the tissues of the animals?

Does not the question naturally suggest itself, *were* they not afraid of proving too much if they experimented with such tissues later on? and that is why we find no mention of such investigations!

THE BACILLUS OF SOUTHERN CATTLE PLAGUE PROVEN.

In the foregoing pages I have placed before the reader all the negative evidences, so far as I know, up to January 1, 1893, published by the investigators of the government, against the validity of a specific bacillus being the cause of the "Texas fever" in cattle, as well as endeavored to show the utter unworthiness and unscientific character of this negative testimony. The reader must himself judge as to whether a case has been made out against the opponents of the investigations published from this laboratory or not. I now enter on a much more pleasant task, that of placing the positive evidence resulting from quite a series of investigations before my readers.

While, I think, nearly every competent investigator will agree with me that we must use common sense and that the fulfillment of certain conditions, such as the invariable presence of one form of germ life in every case of a given infectious disease, if the examinations are made at a time to obviate post-mortal or adventitious complications, still it is always well to fulfill the Koch conditions if we can, and happily we generally can in infectious diseases of animal life, at least as soon as we have learned how to provide suitable artificial cultivating media to the life-necessities of the germ discovered in the tissues of given animals affected with a specific infectious disease.

In regard to the bacillus of the *Southern Cattle Plague* I am absolutely certain that in my investigations every one of Koch's postulates have been as absolutely fulfilled as it is possible to do it, or has been done by him or any other investigator, in relation to any infectious disease of man or beast, the results of which are accepted by all competent men as conclusively proven. It is even negatively admitted by the chief government investigator that bacteria have been found in the blood and tissues of animals diseased with the "*Southern Cattle Plague*," though he denies their value. He says: "All the illustrations which have been published showing preparations of blood from Texas fever animals swarming with bacteria, *and sections showing the same micro-organisms*, are misleading and may be put aside as of no value to the student of this disease." As has been said before, those are "bold words." The failure of one person in any given direction is no criterion by which to judge the success of another in the same direction. Were that so, the majority of scientific acquisitions would still be failures.

DR. STERNBERG ON OUR NEBRASKA INVESTIGATIONS.

In a work which should be authoritative, and which in many respects is a very useful book (A Manual of Bacteriology, by Geo. M. Sternberg), the author takes pains to doubt the reliability of these investigations, merely on the ground that "the investigations of other bacteriologists have failed to confirm the alleged discovery."

Here is what he says in full:

"Billings has announced the discovery of a bacillus in the blood of cattle suffering from Texas fever, which he supposes to be the cause of that disease, but the investigations of other bacteriologists have failed to confirm the alleged discovery. It appears probable that a mistake in diagnosis has been made, and that the disease studied by Billings was an infectious form of septicæmia in cattle similar to the Rinderseuche of German authors. The micro-organism which he describes as coming from the blood of infected animals, resembles in its morphology *Bacillus septicæmiæ hæmorrhagicæ* (No. 61) and, if not identical with this widely distributed species, appears very nearly related to it." P. 528.

The book in question has one great out, it is nothing more than an industrious compiling together of the greater part of the bacterial investigations of the world, without the use of an iota of critical exactness or common sense. It is the blindest of blind acceptance of statements as authoritative, without a particle of pathological consideration. An immense number of bacteriological "finds" are quoted which could better have been left out, as they are of no practical value, and should be classed among the "curios" of bacteriological investigation.

As has been previously said, the fact that other investigators have failed to confirm these results is of no value whatever, and is always so esteemed by all unbiased investigators, especially if the weight of evidence on the other side is of such a positive nature as to fulfill all the requirements of exact and reliable investigation. In this case the author only values the negative evidence and entirely ignores the positive, which shows a bias such as to render his conclusions entirely unreliable. As to an error in diagnosis, if the author had an iota of knowledge of the history of "Texas fever" in the northern states, he should know that such a mistake is a rank impossibility. No one has ever seen any herd disease in our western cattle having the slightest resemblance to the *Septicæmia hæmorrhagica* of Germany. No one has ever seen any such herd disease in cattle in this country except when southern cattle had been brought to our northern pastures. In every case, to

be noted later, of this disease, it has followed the importation of southern cattle to our northern pastures; and in every case I have found one and the same germ, and as often as necessary reproduced the same disease in healthy cattle by the inoculation of pure cultures of the organism. The same has been done with pure cultures of the same germ derived from the abdominal contents and faecal discharge of mature ticks sent direct from southern cattle. The tissues of all the cattle examined are replete in the same germs. Can all this evidence be merely fortuitous? Has any other investigator ever produced more positive and reliable testimony in favor of the discovery of any specific germ?

I have no desire to fight for my so-called "discovery" because it is mine. I have never yet made a claim for priority, and care little about my discoveries, unless they lead to the establishment of a laboratory in which others can carry on this work; but as a pathologist, as one who both practically and scientifically not only knows "Texas fever," but knows his own competency, I cannot see one loophole in the evidence here furnished in favor of the germ here described being the specific cause of the southern cattle plague. I cheerfully admit that the government investigation presents on its bare face a plausible appearing story, but I cannot admit that the evidence is such that its acceptance is warranted. The reader must judge the evidence on both sides, however. There is a "screw loose" somewhere, but I fail to see that it is on my side of the house.

In order to more fully show the utter unreliableness of Dr. Sternberg's conclusion that the germ of the southern cattle plague has anything but the slightest morphological resemblance to that of the German "Rinderseuche," one has but to consider the facts which he ignores in my report. The southern cattle plague germ grows with the utmost reliability and profuseness in and on all the ordinary media, even at a very low room temperature, and retains its virulence indefinitely; at least our tick cultures are now over a year old. At times, from some change in bouillon it has lost in virulence, but a change of medium has soon restored it. It is double the size of the germ of the Rinderseuche. It does not kill rabbits except in immense doses of one ccm., and then requires about a week to do it, even though one drop of a bouillon culture subcutaneously introduced kills guinea-pigs in less than forty-eight hours. On the contrary the Rinderseuche germ kills rabbits in very small doses in less than twenty-four hours. For fuller details the reader is referred to later pages of this work. Dr. Sternberg, however, completely knocks the bottom out of his argument of the relation of

the southern cattle plague to the German organism in his own article, "61," p. 408, to which he refers, and equally demonstrates his bias, carelessness, or incompetency, as well as his having completely ignored our description of the Texas germ.

The following summary of some of the essential "characteristics of *Bacillus septicæmiæ hæmorrhagica*" is taken from Sternberg and placed for comparison alongside the characteristics of the bacillus of southern cattle plague:

BACILLUS OF RINDERSEUCHE.	BACILLUS OF SOUTHERN CATTLE PLAGUE.
1. Is non-motile.	1. Is actively motile.
2. Grows in thermostat better than at room temperature.	2. Grows equally well in room temperature.
3. In gelatine plates, small white colonies gradually becoming yellow-brown.	3. Large colonies, grows much easier—colonies invariably yellow.
4. In punctures grows with a thin white covering on surface, in puncture small isolated or coalescing colonies.	4. Considerable surface growth; grows along whole line of puncture. Secretes a brown pigment. (See Plate I.)
5. Growth on agar feeble.	5. Growth profuse and of a thick viscid character.
6. Fails on potatoes.	6. Profuse on potatoes. (Plate I, Fig. 1.)

This evidence is sufficient to demonstrate the reliability of Dr. Sternberg, an investigator and an authority on bacteriology. It should not be forgotten, also, that he has been a government employe, which is sufficient to account for almost any vagary which may occur in pseudo-investigators.

THE KOCH POSTULATES.

1st. That in the tissues of animals diseased with a specific-infectious disease, a disease which has been long recognized by established clinical and macroscopic phenomena; and when no secondary complications disturb the character of each case, and the animal is either killed or is examined before post-mortal changes can occur, *one and the same species of germ life is invariably demonstrated.*

(a.) By microscopic examination of the tissues of such animals.

(b.) By cultivation in suitable artificial media, by inoculating the same from such tissues.

2d. That the same germ is found in the cultivations, and that on the inoculation of healthy individuals of the same species (not another) of animals from which the germ was obtained when ill with the disease in question, the same disease, in all its clinical and macroscopic phenomena, is again produced.

3d. That in the tissues of the inoculated animal the same germ is

found; that it is again cultivated from those tissues and shown by comparison to be the same organism as that originally derived from an individual having the natural disease.

Now, I say, as I said in the two previous editions of this work, *every one of those conditions have been fulfilled in connection with the bacillus of the Southern Cattle Plague.* Can any one think for a moment that I or any other investigator would have boldness enough to face the verdict of the world with such a positive assertion, unless absolutely sure of his ground? It must not be forgotten, for a moment, that the "great objector" to the correctness of our investigations has been proven to be uncertain and unreliable in nearly every assertion that he has made regarding the specific bacillus of any specific disease in animals in this country, and, with a persistency worthy of a nobler cause, has condemned the work of nearly every other American investigator in the same line to be erroneous. Let us recapitulate a little:

In the Reports of the Department of Agriculture, 1878-1880, Dr. Detmers asserted a bacillus to be the cause of swine plague, and did give a description, not only there, but in the *American Naturalist*, by which any unprejudiced observer engaged in investigating the same disease can recognize the organism in the description. Finical objectors have asserted that Detmers did not demonstrate his find by accurate cultures and inoculations; that he did not fulfill the Koch postulates. That is true. Neither did Brauel nor Pollender, the original discoverers of *Bacillus anthracis*, and yet the world does not deny them whatever honor there may be in having seen the organism first. Even Koch does not now insist on his postulates being so exactly fulfilled, for the discovery of the influenza-bacillus has his endorsement. Where the diseases of man are concerned the postulates cannot always be fulfilled. Common sense must rule in scientific investigation as well as other things in daily life. Let me give a case which I have not published, and, having no desire for priority, shall not mention in detail, as no practical benefits are promised. There is a certain disease in animals in this country in which I can demonstrate one and the same germ in every freshly slaughtered case. I have reproduced the disease in the same species of animals by inoculation with the fresh blood and organs rubbed up in water, and in smears from those animals demonstrated the same organism. *As yet I have absolutely failed in cultivating it.* Suppose I should publish a full description of the disease and its lesions and describe the germ as seen, and the results of such inoculations, would any reasonable inves-

tigator doubt the genuineness of the information? That is more than has been done with the influenza-bacillus and several other organisms the authenticity of which has not been much, if any, questioned.

The primary point in all such cases, where the chain of evidence is incomplete, is the character of the observer for competency and reliability. As an investigator, his qualifications in those two directions are his "stock in trade." They are his "credit" in the bank of original investigation. Now I claim that my opponents have not yet succeeded in establishing a "line of credit."

Let us resume our capitulation, and we shall again see whether this is so or not.

In the reports of Agricultural Department, 1880-1884, the "head of the house" enters into a very energetic opposition to Detmer's "*Bacillus suis*," and claims that a "micrococcus" is the only cause of swine plague. He even goes so far as to enter into a polemic defense of the discovery of this "micrococcus" against the hypothetical claims of Pasteur, and tries to show that Klein (84) had demonstrated the same "micrococcus" in the same disease.

In 1885 the same head says, "that he no longer considers a micrococcus to be the cause of all outbreaks of swine plague," and, though vain endeavors have been made to show identity between that micrococcus and the germ of the new swine plague, unfortunately the only "micrococcus" described in 1885 "fluidified" gelatine, and we know nothing about the other one in that direction.

Then in 1885 the "head of the house," unlike Detmers, gave such a description and illustration of the germ of the true swine plague that its best acquainted friends would not be able to recognize that organism in the description.

In 1890 it is admitted that the germ of the true swine plague was not discovered until late in the fall of 1885, which completely nullifies the "credit" of the "head" investigator's reports on the same subject from 1880 to 1885.

In 1886 the "head of the house" announced a new and widespread "swine plague" in the country, and yet, until to-day, the evidence in favor of the germ then discovered ever causing a local specific disease is scarcely sufficient to establish "his credit" in the house of bacteriological investigation.

The "head of the house" asserts there is no grounds for assuming that the "*Corn-stalk Disease*" is caused by a germ, but the published evidence again disqualifies his "paper on the market."

The "head of the house" also put his "notes on the market," that

a protozoon is the cause of the Texas fever, but on account of totally insufficient endorsement they have not yet been accepted.

SOME USEFUL HINTS AS TO COLLECTING MATERIAL.

The description of the *Bacillus of Southern Cattle Plague* should be next in order, but before entering upon that task it seems well to give a short description of a few technical operations, which any practitioner can easily acquire, for the benefit of veterinarians, especially those practicing in the stock-raising sections of the country. It very often happens that such veterinarians desire the aid of laboratory investigation, and it is even more necessary, in the interests for which such laboratories as this have been and will be established, that the investigators attached to them have the aid of practitioners in different parts of the state, so that a simple practical method of obtaining cultures is also necessary. Such a method has been long in use here, and can be recommended to be as reliable as any exclusive laboratory method, and to do away with the necessity of carrying artificial media into the field, if carried out in detail, as here explained.

Many practitioners undoubtedly desire to know how to make minor investigations, and can supply themselves with a microscope, but have neither the time nor the means to fit up a small laboratory. It has therefore been our endeavor to obviate the necessity of the latter to the greatest degree possible. The method here recommended is only applicable to facultative parasitic septicæmic diseases.

TOOLS.

One needs, aside from the tools necessary to open the animal, a common table-knife, and a small scalpel in a fixed handle; the blade of a pocket knife can always be used, and a piece of platinum wire fixed in a glass-rod, or a steel knitting needle will do in an emergency. The easiest and most practical way of getting a flame to sterilize one's utensils with is to have a small tin salve box, filled with absorbent cotton and an ounce or two of alcohol in a bottle. Wet the cotton with alcohol and ignite it. This is much better than a spirit lamp, gives more flame, and is cheaper and more practical.

As to opening a diseased animal and obtaining cultures from the same:

Where possible, and in this western country it is nearly always so, in order to obtain pure cultures and obviate the probability of the presence of adventitious germs in the tissues of the animal, one should generally kill one in the earliest possible stage of the disease. In some diseases,

like anthrax, one must wait until the last stages to be sure of obtaining cultures from the blood. One must be guided by experience in such matters. This plan also has the advantage of presenting the initial and specific pathological lesions of the disease free from those secondary complications which often lead to mistaken conclusions as to its nature. For complete pathological studies recently deceased animals are better, but not for obtaining cultures.

The animal is placed on its back, and must be either fastened or held in that position. Then the skin is cut through along the middle of the abdomen from between the jaws to the anus and separated from the body, well down on the sides. In separating the fore-limbs from the walls of the thorax care must be taken not to cut across the great axillary vessels of the fore-arm, as all the blood is wanted in the animal, not outside of it; hence animals for this purpose should not be killed so as to bleed them to any extent, when it can as well be avoided.

BLOOD CULTURES, MADE DIRECT FROM THE ANIMAL.

These are called "blood cultures" because the already infested blood of the deceased animal is directly used as a cultivating medium. To this purpose one requires a few one-ounce, wide-mouthed, glass-stoppered bottles, by which the stopple fits over the neck of the bottle like a cap, instead of slipping into it. (The convenience of these bottles in work of this kind cannot be overestimated, and every laboratory should have a supply. They can be easily sent to country practitioners, with directions how to use them, and are equally valuable for personal investigations.) In the place of glass-stoppered bottles the practitioner CAN use any ordinary small, wide-mouthed, cork-stoppered bottle, though it cannot be relied upon for satisfactory results to any such degree as the others. In either case the bottles must be first sterilized. The best and most practical way open to the practitioner is to fill the bottles partly full of cold water and put in the corks, not too tight, then put them into a vessel with cold water, and slowly heat to boiling; this will seldom injure the bottles, if made of good glass; after boiling half an hour, take out the bottles, pour out the water, while-hot, and immediately fill them with alcohol; the cork-stoppered ones should be covered with thin paper, tied on. They can then be set aside, and are always ready for use. The fore-leg is then held at such a distance from the body as to extend the axillary vessels, and the tissues around the latter carefully cut away so as to expose the axillary vein. The jugular vein will often be found more come-at-able and hence can be equally well used. A good flow of blood is easily

obtained by moving the head in an appropriate direction. If the wind does not blow so hard as to make the use of the alcohol flame impossible, saturate the cotton in the tin box and ignite it, and sterilize the outer walls of the vein by gentle application of the flame; too much heat will coagulate the blood in the vessel. Another method, and one which must be resorted to when the wind blows hard, is to saturate some absorbent cotton with corrosive-sublimate solution—1 to 500 aqua, which one should always have with him—and then wrap the vein with such saturated cotton for a few moments. The alcohol is next poured out of the bottles, and it is a good plan to touch a match to the remainder, holding the inside of the cork or stopple to the flame. As soon as it goes out close the bottle. Any little amount of alcohol remaining in the bottle, if all turned out, will not interfere with the result, however. Now take your scalpel and sterilize the blade in the flame, or, when impossible on account of wind, in the sublimate solution (which will ruin that knife, however, but that does not matter), and remove the sublimated cotton, if used, from the vein, and cut through its, then, lower wall, making a gaping wound; hold the mouth of your bottle next to it and it will fill sufficiently as quickly as one can make a transfer with rods, as usually done. *Close the bottle at once.* Much experience has taught me that "blood cultures" made in this way can not only be relied on but have this advantage, that even when the lege-artis cultures are made from the heart's blood and turn out negative from the scarcity of the desired organism in that tissue, that in a few days one may obtain magnificent cultures in these "blood cultures," and if the tube cultures are also made as controls the blood cultures will be found equally reliable. I was gradually led to this method from the trouble I had with the severe winds in the west, as in the majority of cases in studying herd diseases here the animals are so distant from the house (no barns are present) that one can find no protection for a flame. Again, it practically turns out that the danger of getting adventitious flora (fungi) into these blood cultures under such circumstances is less than into tubes. It is, or should be, self-evident that only germs present in the blood will develop in it afterwards, if the bottles are sterile and no accidental germs blow in.

The same bottles can be used for putting pieces of organs in, but in septicæmic diseases the lymph-glands are the most available organs, and those of the inguinal region, or mesentery, the most favorably situated. The way to treat them is to carefully dissect out a whole gland, or, if too large, a suitable end, then heat the outside of the gland in the flame, or, if not convenient, drop into the corrosive subli-

mate solution a few moments, and then place it in one of the sterilized bottles. If there are no pollutions in the gland and all the germs have been killed on the outside, then the specific germ will go on developing within it. I have kept such glands and such blood several weeks in a thermostat, and got nothing but pure cultures out of them, though tested by plate controls.

The next thing to do is to expose the internal organs, which we do by cutting a longitudinal section along the median line, from anterior to the sternum to the anus, taking care not to open the intestines. The covering of the abdominal cavity is then cut crossways from this cut, anterior to the external angle of the ilium, and from the longitudinal cut nearly down to the external ends of the lumbar vertebræ; then the same covering is cut away from its attachments to the last ribs; the thorax is opened by cutting the ribs through on both sides of the sternum, about three inches above it; the sternum is then freed from its diaphragm attachment and clapped back towards the head on the neck. As we have already made "blood cultures" from the veins we need not necessarily have recourse to the heart's blood in this case; but the investigator should also make use of the heart's blood by inoculating tubes, when he can, and the same method of treatment is applicable to the other organs; we will briefly sketch how the heart and those organs are to be treated. The first thing is to expose the right ventricle of the heart, which is done by first opening the pericardium and slipping it back; then the "table knife" is heated, and the outside wall of the right ventricle is seared thoroughly with the red-hot knife; this being done, the scalpel is next heated hot, and the walls of the ventricle cut and burned through with one rapid movement. With the heated and sterilized wire, cooled first, the different media are then inoculated by dipping the wire loop quickly into the blood in the ventricle, and touching the surface of, or dipping it quickly into, the cultivating media.

Now, as to pieces of the liver, spleen, or kidneys—the two first can be operated on *in situ*, the last must be removed. The outside surface is seared over with a hot knife as before and the cut made in the same way; then with the cooled scalpel pieces as large as dice can be cut quickly out at a little distance from the cut surface and quickly placed in the sterilized bottles. If done correctly and quickly, and no adventitious germs are in these organs, the desired specific germ will develop in these pieces, as stated for the lymph glands. The liver is the only one of these organs that is liable of going astray for us, on account of the possibility of germs having been introduced into the gall-ducts by worm-parasites.

Let me say that uncontaminated blood serum can be very easily obtained and retained in a perfectly sterile condition for an indefinite period, and used for cultures in the thermostat at body temperature by a proper technique. As I have never seen this fact mentioned, and it is invaluable as a cultivating medium, I will briefly describe the method which is invariably practiced here for obtaining blood serum. Naturally, everything used must have been thoroughly sterilized first.

The operation is made on the living animal in the same manner as often used in veterinary colleges, except the jugular vein is used instead of the carotid artery. Cocaine is used to anæsthize the skin. A previously sterilized glass rod, to which is attached a sterilized rubber tube, is introduced into the jugular by the operator, who must have sterile hands. An assistant keeps the rubber tube from touching anything and pressed together. A ligature, or clamps, is put easily about the vein anterior to the place to be opened; a longitudinal cut is then made in the side of the outer wall, which has been first washed in the corrosive sublimate solution, the glass tube introduced and well bound; the ligature or clamp removed, and the blood at once allowed to flow into sterilized receptacles. We use preserve jars with metal caps, in which a small hole, to be plugged with sterile cotton, has been cut a little larger than the end of the rubber tube which is introduced in it. *We have never yet had a failure in blood caught this way.*

The balance of the treatment is known to all. The receptacles are placed in an ice-chest for twenty-four hours, and the serum drawn off into tubes, with sterile pipettes as usual. For fluid-serum cultures, the serum is at once placed in small flasks. We have flasks that have been in the laboratory for months, in which the serum was obtained in this way, and they have always remained sterile.

There may be nothing new in all this, but I do not remember of any one's suggesting that blood-serum could be got in this way and never require sterilization, though the serum is now so often experimented with, I suppose it may be. At any rate, no harm is done, and describing the method may be useful to beginners.

One thing I desire again to emphasize, and that is the practicality of cultures made in the field in the above-named manner, for practitioners who desire to send material to laboratories for further investigation. *Very few utensils*, but the utmost exactness in detail is all that is necessary to attain success. This method is far more reliable than vacuum tubes, which cannot always be closed in a suitable manner, and much more practical than wrapping organs or large pieces thereof in sterilized cloths, soaked in some aseptic fluid, as I often do, however, so

as to have all the controls possible, for one need not hurry to the laboratory, and farmers' houses are not always very suitable to work in, nor is it always agreeable. The majority of investigators knew little about making autopsies one hundred or more miles from their laboratory, and sometimes miles from a building, with the wind blowing like a hurricane at sea, and the sun so hot they can scarcely bear it, and flies too thick to mention, or in winter so cold one can scarcely work, thus unprotected; but we have done much of that work, and have found the "blood" and "organ cultures" here described the most practical method open to us, and also that some few practitioners have been enabled to send very reliable materials for culture and experimental investigation when sent the sterilized bottles and directions how to use them.

HOW TO MAKE SMEAR-PREPARATIONS FOR MICROSCOPICAL STUDY.

These directions are, as said, for the benefit of the every-day practitioner who may desire to practice them a little as aids in diagnosis.

Having described how to expose the organs and the heart and take specimens from them, the rest is very simple.

The practitioner takes a wired-rod, if he has one, or his knitting-needle, as the case may be, and sterilizes the wire and the glass-rod for some way, or the needle in the same manner, and allows it to cool a moment and then dips it in the blood of the ventricle, and spreads the blood out in a thin coating on the cover-glass, and lets it dry in the air, and then in the flame, by passing the cover-glass, smear side up, two or three times quickly through the flame.

The liver, spleen, or kidneys, or lymph-glands are treated in the same way, except, if one desires a piece of the organ, a pair of small, slim, thin-pointed forceps are necessary, which must be sterilized in the same manner. Smears of such organs are nothing but a small piece thus taken out of the cut, at some distance from its surface, with the forceps and rubbed out thin on the cover-glass and dried in the air, and in the flame in the way directed. They must not be held in the flame, for that will so destroy the germs so that they will not color well. The cover-glasses are to be held with the forceps by the edge and passed rapidly through the flame.

As to coloring the smears, either a saturated solution of fuchsin or methylen-blue will do. But the carbolic-acid fuchsin solution is equal to the demands of the practitioner in nearly every possible direction he may desire, as it is also as reliable as any stain for tubercle-bacilli.

R.

5 per cent solution acid corbol,	-	-	-	100.00
Alcohol (commercial),	-	-	-	10.00
Fuchsin,	-	-	-	1.00

Filter before using.

To stain smears after being passed through the flame, simply hold the cover-glass, right side up, with the forceps, cover it with sterilized water, holding it level; add three or four drops of the above solution, and hold the cover a few moments; wash the whole off, and dry the glass with filtering paper. The oil of cedar, which is used for the lens, is also the best medium to examine such objects in. As to microscopes a one-sixteenth oil immersion lens is the only reliable one, and, personally, I will not work with less than one-twentieth. Amplifying oculars are a fraud and a humbug. One wants to see what is under the eye, not a picture of a picture. Those who rely on amplifying oculars see germs which bear as much resemblance to the original as reproduced photographs do.

THE BACILLUS OF SOUTHERN CATTLE PLAGUE DESCRIBED.

Before giving a detailed description of the discovery of the germs of this disease and the experiments which conclusively prove it to be the specific cause of the same, we will describe it:

In the former editions of this work it was said that "these organisms are neither to be classed with micrococci nor bacilli; they are not round objects like the former, nor rods like the latter." They belong to that intermediate group to which, for convenience's sake, pathobacteriologists are beginning to give the name of "bacteria." This name is again being generally given up, and all micro-organisms are now called "bacilli," in which the longitudinal diameter is double or exceeds that of the transverse to any marked degree. Hence, we will now speak of the "Bacillus of Southern Cattle Plague" in the future.

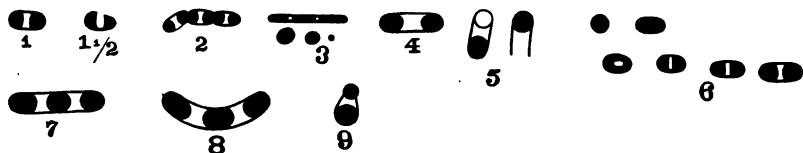


FIG. 1.—Diagrammatic Representation of its Phases of Development.

The longitudinal diameter of these micro-organisms is about twice that of the transverse. They are pole-ended. Their ends are rounded. They are so identically alike in size and shape with the corn-stalk

disease germ that no one could possibly differentiate the one from the other by microscopic examination alone. They are somewhat shorter, less bacillary, and perhaps a little thicker than the swine plague germ.

They color well in most of the analine dyes, especially in fuchsin and methylen-blue. By suitable exposure to the tincture a distinct, uncolored middle piece and sharply colored pole-ends can be easily demonstrated, but if colored too much the middle piece or uncolored belt becomes colored also, and then the germ is seen as a diffusively colored bacillus, or rod. This germ is motile, but it is far more lazy in its movements than either the germs of the corn-stalk disease or swine plague (see Swine Plague Bulletin), though the movements are of the same nature. The illustrations given below (Fig. 2) present as exact a picture of this organism as possible of its appearance in freshly prepared smears from the blood, liver, spleen, or lymph-glands (for these germs can be demonstrated in those tissues, every and all assertions to the contrary), while the diagramic illustration (Fig. 1) illustrates the various phases through which the organism passes in its development.



FIG. 2.—The Southern Cattle Plague Germ.

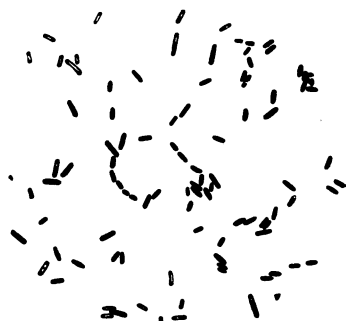


FIG. 3.—The Swine Plague Germ.

The morpho-vegetative phenomena of these bacilli is perhaps even better illustrated in the following cut:

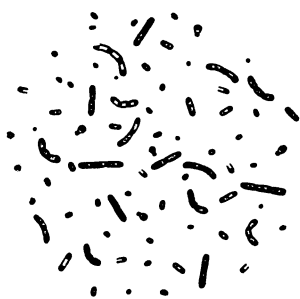


FIG. 4.

Some people have doubted the accuracy of this description. The entire phenomena can all be seen by the prolonged comparative study of "hanging-drop" and dry stained cultures. To one unaccustomed or unproficient in this kind of work, however, the first appearance of a microscopic specimen of a culture of these germs, especially an aged one, would prove very puzzling indeed. In fact, such a person might easily conclude that his culture had become pol-

luted by micrococci, or that some very much larger organism had invaded it. The latter is especially the case when cultures are made on material the surface of which has become quite dry and hard and the condensation fluid all evaporated. The mature form of this and similar organisms has been previously described and illustrated (Fig. 2), and resembles a microscopic bean with its sides and ends so painted as to leave the middle portion of the body untouched as we look down on it. This is the picture which the eye generally receives in smear preparations from the blood or organs and in freshly made cultures on fresh media. A more exact study of cultures, however, especially of "hanging-drops," will show that the above form is by no means the only one in which these bacilli present themselves to view.

Many individuals will be seen which, while ovoid, are much shorter, hence more oval, and the white belt will appear more like a spot in the middle of the object, not extending across it. (Fig. 1, No. 1½). In my opinion, the non-colorable substance, when the germ is treated properly, is a secretion or product of the pole-ends, and in the above form only a small amount has been secreted, and not enough to completely separate or extend the ends. The whole organism is surrounded by a delicate capsule, which must be of the same material as the pole-ends, as it colors with them. The question at once arises, if the whole capsule colors, why do we only see evidence of the same on the sides and not in the portion directly under the eye of the observer, which appears uncolored. The whole capsule colors exactly alike, but is very thin, the substance separating the pole-ends does not color, and the capsule directly under the eye being so thin, we do not see the color, whereas, looking at the sides, we look through more material, which thus gives us more color. The same is true of a thin piece of glass; looking directly through it, it is clear; looking at it edgewise, it is bluish or greenish, as the case may be.

Again, we may see two or three of these organisms united together, all presenting the normal form of full maturity. (Fig. 1, No. 2). In general, however, they appear either singly or in pairs, though in certain media they have a tendency to form long, segmented threads, as well as in some organs. This is applicable to the swine-plague organism better than the one under consideration, so far as I have yet seen. In very old cultures numerous thin rods may also be seen which color diffusely, no uncolored substance being visible in many of them. (Fig. 1, No. 3). The same cultures are also extremely replete in cocci, which leads me to call this condition one of cocciod degeneration. That these are really morphic varieties of this organism is proven

when we transfer such cultures to freshly made material, especially by plate-tests made on such material. This cocci-form represents the first stage in the development of these germs; they are the pole-ends separated from each other, as in dependent organisms. The first step is an elongation of the object. (Fig. 1, No. 4). Then the one-pole end is dropped, giving a free coccus, and then the other becomes free. (Fig. 1, No. 5). In order to correctly observe these phenomena, the addition of a delicate amount of an aqueous methylen-blue solution to the fluid of the hanging-drop is necessary. When one of these coccid ends has been shed the other acts like a plummet, and the other or free end is exposed to view in the drop, and we look down on a round, colorless object enclosed in a delicate membrane which might be mistaken for a spore, but, as the germ is motile and tumbles about, we soon see that such a conclusion would be erroneous. Germs in this condition, with one pole-end in various positions, can be readily seen in dried or stained hanging-drop cultures. They cannot be seen in slides made from cultures grown on solid media.

As said before, I do not know what becomes of the uncolored middle portion, unless it is a fluid secreted by the poles and contained within the capsule, which becomes free, as the pole ends are segmented and is also excluded from the germ into its surrounding media. I still think this substance, the uncolorable, represents the chemical products of the germs, held in solution within the capsule, and that when it does color, we have either changed its character by the heat applied, or that in time the tincture penetrates it. That it has less affinity for the colors than the pole ends and capsule needs no discussion. Again, if we study very exactly, we see this substance first appear as a delicate, colorless paint in the coccid from the moment it begins to lengthen and assume an ovoid form, and it increases in amount until the organism arrives at the full length, as represented in Fig. 1, Nos. 6 and 4. Other forms frequently seen are presented in Nos. 7, 8, and 9 of the same figure, and need no further description.

GROWTH OF THE BACILLI OF SOUTHERN CATTLE PLAGUE IN AND ON DIFFERENT MEDIA.

The germs grow readily at normal temperature, and on the surface of potatoes, when those tubers are not too acid; the growth is generally of a delicate straw color, though, as I said in my previous papers, it sometimes assumes a reddish yellow color, when the colonies become old. (See Plate I, Fig. 1.) The chromogenic proportions of germs, or any other productive property, cannot always be depended on to

differentiate or recognize them by; both media and the age of the germs outside the body cause a very marked difference in these directions. For instance, during the past winter we had some potatoes that had been buried in pits in the ground, to preserve them, which were so acid that none of the germs which generally developed freely on potatoes would grow on them at all. Then the potatoes themselves will affect the color of the colony as well as the time the germs have been cultivated outside of the body.

On the white of eggs, when fresh cultures are used, these bacilli develop as a buff colony, with sharply defined edges, but this color becomes almost white when aged artificial cultures are used. (Plate I, Fig. 2.)

They do not fluidify gelatine, but a peculiarity in freshly derived cultures, which is gradually lost as they become old on agar cultures, but retained in bouillon is that the line of development becomes dark brown, and is embraced in a cloud of dark brown substance of a lighter shade, which is a product of the germs after the cultures are three or four days old. When a very delicate wire is used, the small individual colonies may be seen along the whole line of the puncture if but few germs are on the wire. Quite a growth extends over the surface of the gelatine from the point of the juncture, but as very thin coating. (Plate I, Fig. 3.)

Dr. Thomas Bowhill, of San Francisco, California, also investigated this disease, as will be noticed later on. Recently Dr. Bowhill was in Lincoln, on his way from England to California, and when in the laboratory, looking over a lot of cultures, he at once picked out this germ in gelatine junctures, and remarked: "*That is just the way the germ grew in gelatine which I got from Texas fever diseased cattle in California.*" I refrained from saying anything to him, and he could not have told from my labels what germ it was, as I have special marks for my cultures, for reasons of my own.

On plates the germ is small, round, grayish granulated surface colonies when near the surface, but when deeper situated have a decided yellow tinge.

In bouillon they grow very rapidly, clouding the material and in a few days numbers collect on the surface, forming a coating, but not a mycoderm, which the least jar disturbs, and they sink into the fluid to rise again, if left undisturbed.

On agar-agar they also grow rapidly, but with nothing characteristic, the coating being thick and moist and of a grayish white color.

These bacilli have been time and again demonstrated in sections of

the organs of peremptorily killed cattle, the pieces having been at once cut out and placed in absolute alcohol, so that post-mortal changes or invasion of the organs by any germs not present in the animal at the time killed is simply a matter of impossibility, though the contrary has been claimed in a certain quarter. As has been repeatedly said, *in order to obtain cultures and also the necessary pathological material for control examination, so far as my own work is concerned, in no single case since I began my investigations has such material ever been obtained from an already dead animal; every single one that I have personally used has been peremptorily slaughtered.* The already dead animals, and then only those in which post-mortal changes had not occurred, have been used for pathological studies of the lesions, though cultures have also been frequently taken from them for further evidence. Furthermore, though I have never taken pains to mention it, plate cultures have been resorted to with almost equal constancy. The only weakness, if such it is, in my earlier work, has been that, being in search of specific germs only, and also having then been absolutely without any conveniences, as well as without much money but my own, I did use small pigs almost entirely instead of rabbits, and to-day, when I at once desire to prove a specific germ, I still resort primarily to the species in which the disease naturally occurs. We are better fixed now for small animals, as certain results will show, and in Chicago I had to resort to them often against my will, as the large domestic animals could not be used in the city, and thereby lost quite a number of very important observations, as results in small species of animals, or other than the naturally infested species, are more apt to be misleading than correct, as has been already shown elsewhere. (See Bulletin on Corn-Stalk Disease.)

EVIDENCE THAT THE BACILLUS HERE DESCRIBED IS THE SPECIFIC
CAUSE OF THE SOUTHERN CATTLE PLAGUE.

We have first to describe the conditions in which we obtained this germ. As I have repeatedly asserted, the very first essential in the specific identity of any germ is that it is obtained, uncontaminated, in freshly killed or recently dead animals, afflicted with a specific disease, which has been, and is known for its specific clinical history and course, a disease over which there has not been and is not a doubt as to its actual clinical diagnosis. To my European colleagues I would say that no single disease in man or beast is more absolutely a unicum, more unmistakably easy of diagnosis, than this southern cattle plague when it appears in northern cattle. As the southern cattle which gen-

erally come into the northern-western stock breeding states have almost universally come from Texas, the disease has acquired the name of "Texas fever" among all farmers and breeders in the north, but for reasons given in another place the name which has been suggested as more appropriate is "Southern Cattle Plague." The first thing then in the clinical diagnosis in this disease in northern cattle is that the origin of it was in southern or Texas cattle. It does not take long to find that out; the farmers generally know that at once. The second is that we have no other such acutely deadly disease in northern cattle in this country. The third is its clinical phenomena, high fever, want of appetite, constipation, and hæmaturia in the majority. The fourth is, that it is strictly local, and in all its history has never been known to cross a wire fence to other northern cattle in the adjoining field. A fifth is, the season of the year from May to November, in which it can occur. Severe frosts put an end to it. The pathological lesions are of less diagnostic importance, with the exceptions of the diffuse engorgement of the kidneys, which will never be missed in the majority of the cases of natural infection having the usual course, especially in those which have had hæmaturia (see Plate II); in the more acute cases, especially where hæmaturia is wanting, the kidneys will not present this condition to any marked degree, though the initial stages of capillary engorgement will be present.*

An error in diagnosis in northern cattle can be thus said to be an absolute impossibility to any one competent to judge and make suitable inquiries.

In the previous writing on this disease it was mentioned that I failed in obtaining cultures direct from the spleen of the slaughtered animals, and hence had recourse to the liver principally. It was also mentioned that the first case from which I obtained cultures was at Roca, Neb., in the summer of 1887. I now wish to correct both of these statements, and the reason I did not mention this case in my former reports was that I had forgotten all about it and misplaced my notes, and only found them on my return to Nebraska this time, as well as the slides made from cultures at the time. The cattle belonged to the Hon. John Fitzgerald, a very wealthy Irishman living in Lincoln. Dr. Thomas, the best diagnostician of any veterinarian in the state, had charge of the outbreak as assistant state veterinarian.

I will give my notes just as they were written at the time and are still to be seen in the original record book:

"October 6, 1886. Dr. Thomas brought a piece of the spleen of a

* This lesion is never present in the septicæmia hæmorrhagica of Europe.

heifer, said to have died of Texas fever, to the laboratory. (It was late at the time, and we had to use lamplight). Some small objects, short and round-ended, were to be seen in spleen smears. *Cultures* gave the same object in pure condition. A mouse was at once inoculated with a small amount of splenic pulp. It died in twenty-four hours. Abdominal walls stained yellow-red, as well as pericardium. Spleen immensely swollen. Cultures from the spleen of the mouse gave peculiar ovoid bacteria about the size of the swine plague germ. They grew rapidly on agar in a pearly-whitish color. In gelatine they grew much like the swine plague germ, and extends over the surface from the point of puncture. Does not fluidify gelatine. It seems I then inoculated a rabbit with one ccm. of a twenty-four hours bouillon culture, and that it took five days to kill it, and that it died of septicæmia." As mentioned, this case had entirely slipped my mind when I wrote my reports, which is not to be wondered at considering the continual state of irritation I was kept in during my first period in this state. It may be asked why I did not go further in my investigations at that time? I could not possibly. I was paying nearly all the expenses of my work, and had neither assistant nor servant, even washing the floor in my room, taking care of my animals, and doing all a servant's work, besides I was giving all my attention to swine plague, and out in the country most of the time. I have still slides of the cultures made at that time, and, considering the undoubted character of the diagnosis and the results here detailed, I do not think any one can raise a reasonable question as to the accuracy of the work, hurried and imperfect as it was. It may as well be mentioned that in company with Dr. Thomas and the State Cattle Commission I also visited the sick herd. As it was Mr. Fitzgerald's business and late in the season, and as the sick cattle were closely fenced, I also remember that they were quietly left in the hands of the owner and his veterinarian.

This case then shows this, that a germ growing like and microscopically identical with the one found later on, and one at present in my laboratory from a still more interesting source in pure cultures, was obtained at that time from the spleen of an animal which died of Texas fever, and that there was no error in the diagnosis. Another very corroborative point, which I noticed in my later investigations and have confirmed the past winter many times, was the great resistance of rabbits to this germ, it requiring exceptionally large doses to kill them, while mice succumb very quickly.

At this time my esteemed friend, Dr. Thomas Bowhill, was with me spending all his spare time working in the laboratory, though prac-

ticing his profession also. He afterwards went to San Francisco, Cal., and in the *Veterinary Journal*, London, 1890, vol. XXXI, p. 1, publishes some investigations on the same disease in California, from which I quote the following:

"The first outbreak occurred in October, 1888, and was a very serious affair, one rancher losing 700 out of a herd of 1,200, and this mortality in from three to four weeks. I made six full autopsies and cut open about fifty other carcasses simply to observe the condition of the liver and spleen. In November and December, 1888, I investigated the second outbreak, and made four full autopsies. In February, 1889, I made my third investigation, and performed ten autopsies. It was during this outbreak that I obtained the first cultivations in gelatine and on agar. I could not then define any distinct organisms, as my highest power was only one-eighth of an inch.

"I made my last examination at a dairy, within the city limits of San Francisco, in October, 1889, and as I had now obtained a one-twelfth oil immersion lens I was able to make a thorough examination of cover-glass specimens and cultivations.

"Billings, in his work, claims the cause to be an ovoid-belted organism, and gives experimental inoculations in support of his assertion.

"Dr. Smith says he inoculated rabbits and they remained healthy, but again Billings says, p. 125, he found rabbits to possess natural immunity to at least one-half ccm. injection.

"In October, 1889, I obtained some cultivations on sterilized gelatine from the liver and spleen of a well marked case. Those cultures from the liver developed splendidly, but those from the spleen remained negative. The cultures from the liver did not liquefy gelatine, and preserved a yellowish color at first, becoming darker as they grew older. (See Plate I, Fig. 3.) I also obtained two cultures from the liver on white of eggs as described by Dr. Billings in his work. (See Plate I, Fig. 2.)

"In cover-glass specimens, from the cultures as well as from the liver, spleen, and gall-bladder, an organism similar to the one described by Billings was observed staining most distinctly in fuchsin. A mouse inoculated with two drops of a bouillon culture died in less than twenty-four hours. A cultivation was obtained from the liver of this mouse, which developed exactly the same germ as the original one."

I will say that I have slides from Dr. Bowhill's cultures as well as sections stained by himself from the organs of the cattle, from which he obtained his cultures, and the germ, as described by him, is in them all, and that microscopically it exactly corresponds with my own material.

Attention might be also called to the fact of the extreme mildness of the climate in California in comparison to this section of the country, which accounts for the disease occurring there in months it is never seen here, on account of the cold weather.

As said before, there is no possibility of a mistake having been made in diagnosis, as we have no other cattle disease in this country with the same peculiar clinical history and malignantly fatal course.

We will now return to my own investigations, taking first those cases from which I obtained cultures and later pathological material and demonstrated the germ in the same, and also notice material sent to me in which I demonstrated the germ in one way or the other.

THE OUTBREAK AT ROCA, NEB., SEPTEMBER, 1887.

Fifty-eight head of graded southern cattle were bought by a Mr. F., of this locality, on the 28th of June, of a live stock commission house at Kansas City, Mo. On the 1st day of July they were placed in a pasture with a lot of Nebraska cattle, and on the 29th of August two of the northern cattle died, and another on the following day. Having been informed of this outbreak I immediately visited it in company with Dr. Thomas.

Autopsy 1.—Native steer two years old. Before killing the animal the temperature was taken and found to be 41 degrees C. It was in fairly good condition; had been ill two days, body covered with ticks; visible mucosæ injected and of a yellowish color. Some appetite. Flanks tucked up and fallen in; hair bristling; movements weak and uncertain, especially those of the hind legs. The animal was knocked in the head and its throat cut. Blood quite thin, and while it coagulated, the process proceeded very slowly; it was of a claret-red color, and presented the same when running over the fingers, in fact it seemed more like a red water than blood, not having the usual viscosity of that fluid. On skinning the animal the subcutaneous adipose tissue was somewhat atrophied and of an abnormally yellow color, the abdominal aponeurosis being also tinged with the same color. The muscles of the animal were of a grayish-red color. On opening the abdominal cavity the two first things which struck the eye were the diffuse pinkish redness of the outside covering of the small intestine, and the enlarged and prominent spleen, which was about double its natural size and weighed five pounds; contents rich in blood, somewhat softened, and of a dead red-black color. The stomach and intestines were next removed. The first stomach was two-thirds full of grass with considerable fluid admixed; the contents of the second stomach were still

more fluid, while the third was distended but not over-hard; the fourth stomach was empty, its mucosa was swollen and reddened with small hæmorrhagic centers here and there; it was also covered with a thick, viscid coating, which was closely attached to underlying tissues. Contents of small intestine semi-fluid, and not sufficient in amount to fill the tube; that of the large intestine was of a pultaceous character; the mucosa of the entire tract was swollen and covered with an adhesive viscid coating characteristic of catarrhal conditions; that of the small intestine, especially the anterior two-thirds, was of a bright yellow color, being deeply stained with gall; the posterior portion was of a more deep red color, otherwise in same condition. Liver swollen, and of yellowish-grey-red color; the capillary gall-ducts were so distended that they could be easily seen with the naked eye in many places; the acini were distended. Kidneys swollen and of an intense blueish-red color (see Plate II), though the swollen urinary tubes could be seen through it as grayish-white striæ; the mucosa and fat of the pelvis of the kidney was of a bright yellow color. Bladder about one-half full of a claret-red fluid, which had a specific gravity of 1.15. The abdominal lymph-glands were much swollen and moist; in many of them there were hæmorrhagic centres, while others were of a diffuse dark, bluish-red color. A small quantity of effusion in thoracic cavity. Pleuræ swollen and of a yellowish color, with a few hæmorrhagic points here and there. Fat around heart stained yellow; pericardial sac contained about a tumbler of yellowish colored fluid; its muscle was soft and friable, anæmic and of a yellowish gray-red color. Bronchial lymph-glands much swollen and presented similar conditions to those of abdominal cavity. Anterior and posterior extremities of both lungs presented a mottled and dark red color, and were the seat of numerous centres of lobular pneumonia; cut surface same as outside, but more lustrous and œdematous; inter-lobular vessels engorged; in some the blood was coagulated; these were a secondary complication due to disturbances in the circulation. Mucosa of bronchial tubes swollen, tinged yellow, marked by a few petechiæ; the smallest bronchi, especially those in the diseased portions of the lungs, contained a watery fluid.

Autopsy 2.—This animal was in the same herd, and killed immediately after the necropsy was completed on the previous one. Native grade steer, two years old. As usual it had withdrawn itself from the herd, and stood, near a run, alone by itself. On stirring it up, its movements were tottering and very feeble, unless hazed, when it still had strength enough to do some pretty smart running. A close ex-

amination of the animal was impossible. It frequently tried to pass manure but was unsuccessful, except in very small quantities. Somewhat emaciated, hair rough, eyes had an anxious expression. Although evidently a very sick animal the owner did not feel like killing it, so that we purchased it for the sum of ten dollars. Shot the animal. Immediately on falling took its temperature, which was 42.40 degrees C. Visible mucosæ stained yellow, especially prominent in the conjunctivæ.

No external marking worthy of note. Panculus adiposus considerably atrophied; blood of the peculiar logwood color and character found in this disease. Coagulated and oxidized slowly on exposure to the air. Peritoneal cavity contained about six quarts of an amber-color fluid. Costal peritoneum swollen and clouded, and marked by numerous petechial spots. Serosa of small intestines of a diffuse pinkish-red color; some few small hæmorrhagic centers present; that of large intestines clouded, swollen, grayish-red in color, with numerous petechiæ here and there. Spleen exceedingly swollen, contents semi-fluid, weight four pounds, and covered with vegetations in various places. Gall-bladder distended and full of greenish-yellow fluid. Cut surface of liver somewhat anæmic; acini very much swollen, general color being yellowish-gray-red—intra-acinous vein invisible; gall capillaries presented a beautifully injected condition. Kidneys swollen; capsule non-adherent; external and cut surface of cortex being of a diffuse logwood red color (see Plate II); the medullary substance also a diffuse red, but of a lighter shade; mucosa of pelvis swollen and replete in hæmorrhagic centers of different dimensions. The bladder contained about a quart of claret-colored fluid; its mucosa was much swollen, vessels injected; many small hæmorrhagic centers present. Nothing need be said of the first three stomachs, but the fourth offered a fine picture of the lesions seen in this disease. It was empty; mucosa very much swollen and of a very intense, deep, but not dark, pink-red color, the diffuseness of which was interrupted in many places by hæmorrhagic centers, and in others were spots covered with grayish-yellow material, somewhat dry, and which, on being removed, exposed an ulcerated surface, which was below the line of the surrounding tissues; the edges of these ulcerations were irregular and swollen; the balance of the mucosa was covered with a viscid adherent coating; vessels of sub-mucosa intensely engorged; numerous hæmorrhages being also present in these tissues; towards the pylorus the mucosa was of a deep yellow tinge, which extended throughout the duodenum into the jejunum; contents of the latter semi-fluid in character and dirty yellow in color; mucosa swollen, presenting similar conditions to those described in the stomach.

Large intestines: Contents semi-fluid and of a dirty yellowish-green color; mucosa swollen and of a light pink color; numerous hæmorrhagic centers here and there, which became more profuse and diffuse, as well as extended as the rectum was approached; towards the anus the entire mucosa was a dense dark-red color and very much swollen; the crests of the folds were marked by small centers covered with a dry, yellowish-gray material. No effusion in thoracic cavity; lungs comparatively normal; myocardium opaque, anæmic, yellowish-gray-red in color, and friable. Lymph-glands of entire body swollen, œdematous, and of a diffuse pink-red color.

It was from these two autopsies that I first obtained the germ of the Southern Cattle Plague. The method previously described of obtaining cultures direct from the blood had not then been much used by me, and its reliability not known, as it is now, so, being eighteen miles from the laboratory, a large piece of the liver from each animal was at once cut out, as soon as that organ was exposed, and wrapped in a towel saturated with a strong enough solution of carbolic acid to corrode the outside, and the whole put into a glass preserve jar, out of which the carbolic acid solution had been poured after taking out the cloth. Objections have been made to this method, but it has worked, and still works, very reliably in my hands when the material is at once taken to the laboratory and cultures made, and I do not think such objections justifiable. Attention has been previously called to the fact that tubes cannot always be used in the field on account of the strong wind generally blowing here, and the extreme heat and flies in summer are also an obstruction. In this case the nearest building was five miles. On completing the examinations we at once drove back to Lincoln and were at the laboratory in an hour and a half; tubes and bouillon flasks were at once inoculated, and ground squirrels also, a most convenient animal when we can get them here in the west. Plates were also made. The organism herein described was found present in all the cultures, and killed the squirrels, and was found in their blood.

SOUTHERN CATTLE PLAGUE PRODUCED BY INOCULATION WITH PURE CULTURES OBTAINED ABOVE.

As soon as the purity of the cultures had been determined by plates and microscopical examinations it was decided to at once test the specific value of the bacillus obtained by inoculating some healthy cattle. This was done on Saturday, September 10, 1887, a red cow and a five months' old black steer receiving 10 ccms. subcutaneously in the dew-

lap. The cow was a very wild animal and it was impossible to catch and handle her again for examination, but she fell off on her feed and condition, became constipated, but finally recovered.

The black steer was off its feed, hair bristling; the animal stood much by itself; respirations accelerated, but not labored; temperature elevated. First observed to be unwell on Wednesday, the 14th; temperature, 4 P. M., 42.50 degrees C.; Thursday, 15th, temperature, 4 P. M., 42.25 degrees C.; Friday, 16th, 41.50 degrees, 9 A. M.

NECROSCOPICAL NOTES ON BLACK STEER INOCULATED WITH PURE CULTURES OBTAINED FROM THE ROCA CATTLE.

Seeing that this animal might recover, and desiring to observe the lesions produced by the inoculation, and also to oblige my friend, Dr. C. C. Lyford, of Minneapolis, who was visiting me and had never seen an autopsy in a case of the Southern Cattle Plague, it was determined to kill the animal. The fact that the same micro-organism as that injected was obtained from its tissues in pure cultures, and a comparison of the following necroscopical notes, with those of the original cattle, and others following from a steer from another outbreak, should convince any unprejudiced person of the reliability of these investigations and that Southern Cattle Plague is caused by specific bacteria. Blood of the peculiar color and lac consistency, which is more or less characteristic of this disease. (Some freshly flowing blood was caught in a sterilized bottle as it spurted from a cut artery and the same micro-organisms afterwards found in it.) Panculus adiposus somewhat atrophied and of a decidedly yellow color; costal peritoneum and omentum, as well as the serosa of the large intestine, were of the same color, interrupted by numerous petechiæ. Small intestine of a general diffuse pink-red color, variegated by engorged vessels and a few petechiæ; blood-vessels of mesenterium engorged; lymph-glands swollen and oedematous; interstitial and sub-capsular vessels injected. Other lymph-glands the same. Abdominal cavity contained about two quarts of straw-colored fluid. Spleen swollen, full of blood, and soft, weight two and a quarter pounds. It must not be forgotten that this animal was but five months old. Liver swollen; edges rounded; central vessels of acini invisible, while the inter-acinous vessels were injected; each acinus was most beautifully decorated by delicate lines of a bright yellow color, which represented distended inter-acinous gall-ducts. Gall bladder distended. As I believe this pathological injection of the gall capillaries to be, perhaps, pathognomonic of this peculiar infectious disease in cattle, I desire to call attention to a necroscop-

ical remark on the same subject made by the government investigators in connection with the cattle which they claim to have killed by placing on them young ticks artificially hatched from collected ova; they say: "Liver weighs about twelve pounds; enlarged; yellowish in sections. Complete injection of the intra-lobular bile capillaries." (Report Bureau, A. I., 1889-1890, p. 98.)

It is remarkable that both ticks and germs should produce the same pathognomonic lesion in the same disease, and yet in their cases no germ exists; and none were looked for by their own records.

Here is a very weighty piece of testimony against the young-tick-protozoan inoculation theory of the government, given by its investigators:

"These brief notes demonstrate that Texas fever can be produced by placing young ticks on cattle, and that the disease cannot be due to any abstraction of blood, for the ticks were still quite small, and had scarcely begun to draw blood on a large scale." (How then could so many protozoa have gotten into the blood of the cattle as to kill them under those circumstances? And, as has been said before, if the ticks drew blood, and the young ticks in this case having been artificially hatched from collected ova, and the young ticks never having been in any relation with diseased cattle, or, according to these investigators, the possibility of the same being absolutely barred, how then can the protozoa have gotten from the young ticks into the blood of the cattle and caused disease under those circumstances? The whole order of the tick economy must have been reversed. Instead of sucking in blood, they must have stuck their probosces into the skin and vomited out protozoa. No other way of protozoa-inoculation is possible according to the government theory of tick transmission.)

Kidneys swollen; outside surface most beautifully marked by the injected condition of the inter-tubular vessels and presented as fine a picture of natural injection as could be desired, as well as illustrates the earliest stage of that general renal engorgement which is represented by the intensely swollen, bluish-red kidney (see Plate II) so often seen in this disease under conditions of natural infection.

Stomachs partly full. Mucosa of the fourth stomach very much swollen and of a diffuse dark pink-red color, interrupted by numerous engorged vessels, ecchymoses and diffused hæmorrhagic centers. Mucosa of duodenum and jejunum swollen and of a yellowish-red color: Peyer's patches and solitary follicles much swollen; large vessels engorged; occasional hæmorrhages in the mucosa; contents semi-fluid.

The yellow-staining of the mucosa of the anterior part of the small intestine was lost in the ileum, though here the membrane was also swollen, but of a diffuse pink-red color. Mucosa of large intestine swollen and of a delicate diffuse pinkish-red color; some engorged vessels present; this condition increased in intensity from the beginning of the rectum towards the anus, where the mucosa was intensely swollen and of a dark pink-red color, with small hæmorrhagic centers, especially on the crests of the rugæ; contents more and more solid until they become quite hard in the posterior portion of the rectum. Nothing abnormal in the thoracic cavity except that the lungs were slightly hyperæmic; the bronchial lymph-glands were swollen, red and cedematous. Myocardium opaque, anæmic, yellowish-red in color and somewhat soft. Mucosa of trachea swollen, vessels injected, some few petechial centers. Bladder half-full of straw colored urine; vessels of mucosa somewhat engorged.

Smear preparations from the blood and organs gave the same bacteria injection, as well as did the cultures from the blood and organs which were carefully compared with the originals.

In comparing the virulence of bouillon-cultures from this calf in ground-squirrels, the same dose was found to be less actively fatal than that of the original material. This I do not consider at all remarkable, as the land of the southern states and not the bovine organism is the natural habitat of this parasite.

Repeated examinations of sections of the organs of all the animals mentioned here have since been made and always with positive results. Again, in all cases, the pieces from which sections were to be made were cut out ere the animal was cold and immediately placed in absolute alcohol.

It is impossible for me to conceive how any man could more exactly fulfill every condition of exact investigation more completely than it has been done above. The most rigid examination of the lesions induced in the inoculated animal with those in the natural diseased cattle must show the most exact reproduction in every particular, except that the circulation disturbances in the kidneys of the young inoculated animal were not so advanced as in those from which the germs were obtained, but the "mahogany" kidney, while very common, is not an absolutely necessary phenomenon in this disease.

It must be mentioned that there were no ticks on or about the inoculated cattle, and no diseased cattle nearer than those in Roca, eighteen miles away, so far as known.

Autopsy 4.—It would seem as if further evidence were absolutely

unnecessary, but I hope in this edition of these investigations to so place the bacterial etiology of the Southern Cattle Plague before the world that no one will dare question its reliability, and thus have prepared the way for a more brief description of the disease. This animal is from the second outbreak at Tekamah, Neb., the latter part of September, 1887, and from this animal's blood and organs pure cultures of the same micro-organism as in the others were obtained and subjected to every necessary control and test. Small pieces of the organs were also placed in absolute alcohol while the animal was still warm.

It may be as well that I mention here that I did not obtain cultures from the first Tekamah outbreak, having tried in vain to inoculate tubes in a hurricane, with a hot, dry, dusty wind blowing, and obtained a complete set of the fungus flora rather than germs. The tissues, however, were properly placed in alcohol, and the same germ can be seen in them by any one competent to examine them.

Now to our steer. Red and white steer, two years old, very good condition. Temperature 42 degrees C. Respirations rapid and distressed. Eyes fixed and becoming glassy. Pulse very rapid and weak.

Cut its throat. Blood of a peculiar reddish color, having more the character and appearance of some coloring fluid than normal blood. It coagulated but slowly on exposure to the atmosphere and did not become scarlet-red as quickly as most blood does under the same circumstances. Panculus adiposus well preserved. Superficial inguinal glands intensely swollen and of a diffuse reddish-gray color; cut surface moist and glistening; a red, watery fluid exuded on pressure. The other lymph-glands in the body were in the same condition; those of the mesenterium being the most excessively so, and replete in blood. The abdominal cavity contained about a quart of a yellowish-colored fluid. Peritoneum also stained with the same color. The outside of the small intestines presented a bright diffuse pink-red color, while that of the larger was more grayish-red. The spleen was much swollen, and presented a peculiar variegated appearance, the superficial veins being intensely engorged, and the trabeculæ showing through the capsule. Weight, six pounds. It was twenty-one inches long, eight wide, and three thick in the middle portion, which was much more swollen than at either end. When cut it was found softened and completely engorged with blood which flowed out of the substance of the organ.

I did not stop to remove the liver, but at once cut out a piece on opening the animal and wrapped it in three clean napkins that had been

soaked in five per cent carbolic acid solution for over a week. The whole organ was very much swollen, its edges thick and round; gall-bladder distended and full of a greenish-yellow gall; cross sections demonstrated the liver to be quite full of blood, inter-acinous vessels engorged, acini distended and yellowish-gray in color; gall capillaries markedly injected, and could be seen as delicate hair-like lines of a yellow color working the limits of each acinus. Kidneys intensely swollen, the left one weighing two pounds. Fatty capsule the seat of more or less extensive hæmorrhages; capsule non-adherent; outer surface of a diffuse, dense logwood, red, some of cut surface (Plate II); medullary substance could not be distinguished from the cortical so far as any difference in color went; mucosa of pelvis swollen and the seat of numerous hæmorrhagic centers; cavity contained a quantity of coagulated blood which was attached to the mucosa. *Bladder*, two-thirds full of claret-colored urine; mucosa swollen; vessels engorged with numerous diffuse and small circumscribed hæmorrhagic centers scattered through its substance.

Stomachs. First three comparatively normal. The fourth contained a small quantity of gall-stained, greenish-yellow material. Mucosa intensely swollen and of a diffuse livid pink color, interrupted by the engorgement of the larger vessels and numerous hæmorrhagic centers of various size and contour; aside from these were numerous denuded spots with swollen thickened edges, the base being of a reddish-gray color; the entire mucosa was covered with a viscid-catarrhal coating which was of a yellowish color in the pyloric region. Mucosa of small intestine much swollen and of an intense yellowish-red color, interrupted by numerous hæmorrhagic centers. Solitary follicles and Peyer's patches swollen, latter markedly. Contents semi-fluid and of a dirty yellowish-green color. Mucosa of large intestine still more swollen and of a deep-red color; hæmorrhages more extensive and frequent; these conditions increased towards and in the rectum where the swollen folds were marked; towards the anus the hæmorrhagic engorgement of the mucosa were still more extensive, being of a diffuse pink-red color. Contents pultaceous and of a yellowish color, stained with blood on the surface.

Thoracic cavity free from effusion. Pericardial sac held about two tablespoons of reddish-colored fluid and covered with petechiæ. Myocardium opaque, anæmic, friable. Bronchial lymph-glands like others. Lungs normal; mucosa of trachea and bronchi somewhat swollen, numerous petechiæ. Covering-glasses were at once smeared with the first blood caught in a sterilized bottle, also from the liver

and cultures made on agar on return to town. In the blood, and also the covering-glasses prepared from the liver, the same germs here described were present in great numbers. Cultures of the same also developed.

The observations, thus far recorded, were made in 1887. They were again confirmed in every detail, with the exception of cattle inoculations, by autopsies and examinations of slaughtered animals in outbreaks at Gibbon, Neb., in August, and at Plattsmouth, Neb., in September, 1888. About the same time, my colleague, Dr. R. J. Withers, in Chicago, sent me some carefully collected fresh and alcoholic material from cattle that had perished in certain experiments then being carried on at the stock yards at Chicago by the Illinois Live Stock Commission. By means of plate culture and the squirrels, the same germ was again isolated from this material and is at present in the alcoholic material.

To sum up, then: One and the same micro-organism has been isolated in pure cultures, from the blood and organs, and in alcoholic material in an outbreak of the Southern Cattle Plague at Roca and at Tekamah in 1887, in the same manner in outbreaks at Plattsmouth and Gibbon, Neb., in 1888, and also in material sent from the experiments made by the Illinois Live Stock Commission the same year, and the disease has been produced in a young steer by the subcutaneous inoculation of pure cultures of this same germ, and pure cultures of the bacillus thus found; again obtained from the blood and organs of the inoculated steer, and demonstrated in alcoholic specimens of the same. The same germ has also been demonstrated in all the details (except the cattle experiment) by Dr. Thomas Bowhill in San Francisco, from the blood and tissues of sick cattle.

Is the bacillus of anthrax, of tuberculosis, of glanders, any more conclusively proven than this of the Southern Cattle Plague?

In the face of such evidence as that, however, we find the chief of the Bureau of Animal Industry, an individual absolutely without credit as an investigator, and an officer of the government of the United States, telling the world that:

"I am now prepared to say that Texas fever is not a bacterial disease. All the illustrations which have been published showing preparations of the blood from Texas fever animals swarming with bacteria, and sections of tissue showing the same micro-organisms, are misleading and one of no value to the student of this disease. The blood and tissues do not present such appearance when properly examined."

The evidence is all here. The bureaucratic head admits that illustrations of blood swarming with bacteria and "sections of tissue showing

the same micro-organisms" have been made. Such things then exist. Furthermore, they have had them and seen them in Washington. They do not dare deny their existence, or that they were obtained from cattle suffering from Texas fever. All they can say is, "that the blood and tissues do not present such an appearance when properly examined." Who is to judge of the "proper" in this matter? Who gives the verdict? A person notoriously known not to have done an iota of scientific investigation in his life, or one in whose work the most searching reinvestigation cannot and will not find a single material error? Error, so far as the bacilli of the Southern Cattle Plague is concerned, is a thing not to be thought of for a moment in these investigations. The evidence of the autopsies, made on animals purposely killed, and when the flesh was still quivering, "blood cultures" made from the blood caught directly in sterilized vessels (this is how the swarms of the "same bacteria" as demonstrated in the sections were produced) controlled by cultures from organs made in tubes within an hour, during which time the piece of organ was enclosed in cloths saturated with five per cent carbolic acid; sections of organs made from small pieces cut out while the organ was still warm and the heart often beating and placed in absolute alcohol. Pure cultures obtained and the same disease produced by inoculation and cultures again obtained and all this confirmed by other outbreaks and material collected with the same rigid exactness and finally confirmation by Dr. Bowhill. What disease of the human brain could lead any person to such imbecile assertions as the efforts of the department to strengthen itself in the eyes of the "fool farmers" by trying to shake our work by misrepresentation? It is my unpleasant duty to still further open up the ulcer of political corruption in the farmers' department of the government of the United States.

THE BACILLUS OF SOUTHERN CATTLE PLAGUE IN THE TICKS.*

Nearly every falsehood or misstatement has to have a microscopic fragment of truth in close proximity if it is to have any vitality whatever.

That is true of the germ claimed by the government to be the specific cause of a second widespread plague in our hogs. The germ is ubiquitous. Of itself it seems to be harmless, as it has not been found, alone and by itself, causing the death of a single animal by the gov-

* I am informed on good authority that a similar attempt is to be made by the Department of Agriculture to discredit the bacillus of the Corn-Stalk Disease. Let me here say that I have never published one-half the positive testimony on that bacillus and disease, considering it unnecessary to overweight the evidence. He who can't find both these bacilli must be either a fool or a rascal.

ernment since it first announced the second plague in 1886. The true germ was in, or had been in, the same hog, and prepared a feeding ground for the fraudulent germ. That is all they have proven. The same is true of the ticks. In fact, several factors are often present together. Southern cattle, southern ticks, and the disease in northern cattle. But the ticks are not present in all cases on our northern cattle. There were no ticks present on the cattle in the second outbreak at Tekamah, Neb., in 1887, and none on the gray steer at Roca, though on the other one. The dictum, "No ticks no Texas fever," will not work. It should read "No sporozoa taken on tick." Having demonstrated, beyond the power of man to successfully or honestly contradict, the presence of a specific bacillus in the blood and organic tissues of cattle afflicted with the Southern Cattle Plague, it now becomes my most agreeable duty to show to the world the real part which the ticks can play in the historic genesis of this American cattle pest. While we shall speak of ticks, no one need take our "notes on tick." Their endorsement, with facts, will be so strong that they will be taken "at par" by any intelligent microbe broker in the world.

There is nothing new in the fact that southern cattle are frequently if not generally, infested with ticks when they come north in the summer months. There is nothing new in the hypothesis that the "ticks" might possibly be in some way connected with the etiology of this disease. The assumption is comparable to that of corn-smut being the cause of the corn-stalk disease. The smut in that, the tick in this disease, were something tangible, and that which is before the eye and most marked is generally the thing blamed and the real sinner is seldom seen. The tick hypothesis really had more solid foundation to stand on before the government united with it its absolutely ridiculous protozoan theory than it has now. But as in the "smut" assumption there was, and is, something in corn causing disease, so in the tick hypothesis there is a truth which the people in Washington have either not seen or willfully hidden from public view. The ease with which the thing was developed in our hands would seem to justify either malicious neglect or dishonesty on the part of the government investigators.

THE HISTORY OF ONE TICK AND WHAT IT REVEALED.

The fate of a battle has often been decided by a single shot. The fate justly due the investigators at Washington may result from the investigation of a single tick.

On Sunday, October 4, 1892, I received a box of ticks from Dr.

Tait Butler, veterinarian to the agricultural experiment station of Mississippi, the ticks having been picked off the cattle of the station, put in a box and sent me. The box was an oblong wooden one, with a wooden cover and not overclean inside. In it were quite a number of the large, grayish ovoid bugs known as southern cattle ticks. The weather was unusually and desperately cold for the time of year. They were at once put into the thermostat to see if it would revive them, but it did not seem to. They were all shriveled up the next morning except one, which was still a full, plump, and rather lively individual for a tick under such abnormal conditions.

In the box were, besides the ticks, roundish, irregularly surfaced masses, somewhat like grape-shot, about the size of a small pea, of a brick-red color, and looked like masses of stuff made in pellets and then stuck together. The stuff was the faecal discharges of the ticks. There were no ova about them. It was not the season of the year for them, perhaps. I might as well say that I did not believe much in the tick business any more than I did in the "smut" hypothesis in the corn-stalk disease, and had it not been for numerous friends around me at the time, I think it very probable, ticks, box, faecal discharge, and all would have gone into the fire. In order to gratify my friends' curiosity, I first consented to examine the "little red balls," as one called them, so one was broken open and a small piece taken from the center, and rubbed upon a cover-glass with sterilized water, and then stained with fuchsin and put under a microscope. My indifference was at once changed to most intense interest, for in this stuff there were millions of an ovoid belted germ so exactly like those I had obtained from every outbreak of Texas fever that no mortal would ever have doubted their identity, knowing that these ticks also come from southern cattle. It will be remembered that southern cattle are seldom, if ever, ill themselves when capable of disseminating a death-dealing influence to our northern stock. The reason of the frequent failure to find the germs in southern cattle, which have gradually become "Gift-fest," immune, by the fractional introduction of the germ while young animals, is ignored by some American investigators, but is well known to every competent investigator.

The next step was to examine the ticks, the plump, lively one being selected. The tick was first given a plunge-bath in corrosive-sublimate solution, then one in alcohol, and a final one in sterilized water; it then had its back split open with sterilized scissors and out of the wound oozed a reddish-black, thick stuff of tar-like consistency. Smears of this material were at once made and an apparently pure condition of the

same germ that prevailed in the fæces observed. Cultures in bouillon gelatine, on agar, and plates were made from this stuff, and, most singular to say, were so pure that no other germ could be detected except a small rod and a coccus in three or four colonies each on the four plates. Plates were then made from the bouillon, gelatine, and agar and with approximately the same result. The bouillon flasks and several gelatine tubes were mostly pure, the agar had a few other colonies as above mentioned. The red balled fæcal discharge, from six of the masses, a small fragment taken from the center being used, were then inoculated into the same media, with approximately the same result. All other germs but the one mentioned were discarded. The germs thus isolated corresponds in every particular with the one obtained in previous investigations from the blood and organs of cattle peremptorily killed in order to make studies in the Southern Cattle Plague. There is no need of repeating the descriptions.

Here then we have several positive discoveries as against the negative assertions of the government that "Texas fever is a disease not caused by the bacteria," etc.

1st. Bacteria found in every outbreak of the disease in northern cattle to which we have had access or from which material has been sent us.

2d. The disease produced in cattle by inoculation with the germ as obtained from cattle.

3d. An identical germ found in the abdominal contents and fæcal discharge of ticks direct from southern cattle.

The results conclusively demonstrate either insufficient investigation on the part of the government investigators, or the suppression of the true facts.

But we all know that germs from two different sources can look alike and grow so very nearly alike as to be quite deceptive—though it must not be forgotten for an instant that in these cases *we have a common origin for the germs.*

1st. In the blood and organs of cattle infected on pastures previously infested by Texas cattle, and in a steer inoculated with pure cultures from that steer.

2d. In ticks and their discharges which had been taken directly from the bodies of southern cattle.

The point to be at once decided, and in such cases as this I do not allow much dust to collect, so as to at once see what would be the effect of

THE INOCULATION OF NORTHERN CATTLE WITH PURE CULTURES FROM THE TICK.

October 8, 1891, 11 A. M. I inoculated two calves, one a six months old Jersey, tough, rugged, and stunted; the other smaller, a six weeks old good shorthorn, fat and sleek. Both received ten ccms. of a pure bouillon culture, of the same organism derived from the tick, into the jugular vein.

4 P. M. Also inoculated a full grown guinea pig one-half ccm. and a full grown rabbit one ccm.—same bouillon culture. Made plates from bouillon used in these inoculations; the result gave pure cultures. Carried all the various cultures in and on different media for some time, and then reduced them to bouillons, using gelatine cultures and occasional plates with a test of virulence, in guinea pigs occasionally.

9th. Guinea pig died at 5 P. M., blood full of the ovoid belted germ, pure cultures from heart's blood.

11th. Rabbit died this morning. The big dose one ccm. in the rabbit, and the quick death of the guinea pig and slow course in the rabbit, gives another confirmatory point between this organism and the one found in cattle. Rabbits are not immune against the action of this germ, though so to the same doses used in ground squirrels and guinea pigs, but all my inoculations show this can be overcome by unreasonably large doses. The examination of the heart's blood of our rabbit, and plate, and other media of cultivation, demonstrated no other germ present in the blood than the one injected.

THE RESULT OF THE TICK-GERM INOCULATION IN THE CALVES.

October 9th, 3 P. M. Went out to the farm at the request of the superintendent, who reported the germ injected to be chain-lightning, and, much to my astonishment, for I did not expect such speedy action, found the six weeks old calf moribund. It seemed as if "fire had struck it," it had fallen away so. All the morning it had been severely constipated and strained; its muffle was dry as parchment and covered with a whitish, mass-like salt; breathing had been very rapid; had not eaten since inoculated, so far as known; would drink frequently, but little at a time.

NECROSCOPY ON CALF KILLED WITH TICK-GERM CULTURE.

As the animal was moribund, waited until quiet, then at once made autopsy. Skin carefully removed, "blood cultures" made, as directed, from the axillary vein. Took cultures from heart's blood, liver, and

spleen. Pieces of organs at once put into alcohol before the animal was cold. Sub-cutis somewhat emaciated; vessels very much engorged; very many petechiæ scattered about, and an occasional diffuse hæmorrhage. Blood excessively thin, though dark colored, as in all previous cases; as is the great characteristic of the Southern Cattle Plague, it resembled some watery dark-red dye stuff far more than blood. This is best seen by letting it run over the fingers; it thickened, but formed no solid coagulum on exposure to the air while the autopsy was being made; also reddened very slowly. Superficial lymph-glands intensely swollen. Costal peritoneum and omentum dotted with innumerable petechiæ and ecchymoses; about a quart of straw-colored fluid in abdominal cavity; serosa of small intestine of a diffuse dark-red, dotted by great numbers of still darker petechial and more extensive and less sharply defined hæmorrhagic centers. Large intestine of a more gray color, with extensive diffuse hæmorrhages and very few of a small, circumscribed character. Mesenterial lymph-glands immensely swollen, œdematous, vessels engorged—many hæmorrhagic centers in substance. Spleen excessively swollen and full of blood. Liver much swollen at base. Acini very much disturbed and mostly of a lustrous yellowish-gray color; cut surface dry and anæmic; gall capillaries beautifully injected and very prominent; gall-bladder so distended as to resemble a blown-up bladder. Outside of second and third stomach fairly dotted with petechiæ, with some swelling of inside lining and a scattered hæmorrhage here and there. Mucosa of fourth stomach excessively swollen, of a diffuse dark-pink-red color relieved by numerous petechiæ; ecchymoses and more extensive and less limited hæmorrhages of a much more intense color; a profuse, grayish, viscid coating covered the whole mucosa. Towards pylorus the coating was intense yellow, while the underlying tissues were deep red; mucosa of duodenum corresponded thereto; jejunum more and more red towards ileum, which was again intensely red; follicles and patches markedly swollen. Lungs normal, except punctiform hæmorrhages scattered through pleura; some aqueous effusions in air tubes, mucosa of which some swollen, many petechiæ. Bronchial lymph-glands same as as mesenteric.

The "blood cultures" from the axillary vein were at once placed in thermostat to test and demonstrate their value to find what was present, and were found not only pure by plate controls, but also so replete as to probably explain the mystery to the uninitiated in the technique of field investigation, the "slides from Texas fever diseased cattle swarming with bacteria." The cultures from the heart's blood and

organs on ordinary media were left at room temperature, subjected to every possible control, all being tested side by side on eggs and potatoes with the original and giving the same results.

SECOND CALF, STUNTED JERSEY, SIX MONTHS OLD, INOCULATED
WITH TICK-GERM CULTURES.

This calf was very sick, became very much emaciated, muffle dry and covered with a white scale as in the other case. It finally recovered and was eventually killed by an intravenous injection of the bacillus of the corn-stalk disease, the results of which are detailed in the bulletin on that disease.

Clinical observations:

Temperature October 9, 11:40 A. M. 39.80 C.

Temperature October 10, 7:00 A. M. 40.00 C.

Drank a little, but ate nothing.

Temperature October 11, 7:00 A. M. 40.60 C.

Ate a few oats and drank little.

Temperature October 12, 7:00 A. M. 41.20 C.

Temperature October 12, 8:00 P. M. 42.60 C.

First excrement passed since the 9th, very dry and lumpy, with blood clots on it. Urine very dark red.

Temperature October 13, 7:30 P. M. 41.30 C.

Excrement very hard, passed in small quantities covered with slime and blood. Urine still dark red. Calf not eating anything to speak of, respiration quick and calf getting emaciated, cannot get up without help.

Is this the Southern Cattle Plague or not?

Temperature October 14, 7:35 A. M. 40.10 C.

Temperature October 14, 8:00 P. M. 40.40 C.

Temperature October 15, 6:10 A. M. 40.35 C.

Temperature October 16, 7:00 A. M. 39.80 C.

Temperature October 16, 8:00 P. M. 40.10 C.

From this time on the animal slowly recovered. While sickest, some of the hard manure covered with slime was taken to the laboratory and a small amount, not larger than a good sized pin's head, was rubbed up with sterilized water and one-fourth ccm. was injected under the skin of a full-grown guinea-pig. It died in less than twenty-four hours, being dead, though not quite cold, when we got to the laboratory next morning, the exact time cannot be given. Pure cultures of the original germ

injected into the calf were obtained from the blood and plate cultures from the manure gave a preponderance of the same germ.

About this time I went to Chicago and was away until about December 10.

On December 31, 1891, I tested the original cultures from the tick on a guinea pig, and, very much to my surprise, some unknown factor had materially mitigated their virulence.

This guinea-pig died at 6 P. M. on the 9th day of January, 1892; pure cultures of the same germ were again obtained from its blood.

January 2, 1892. Two more guinea-pigs were again inoculated from the same culture from the ticks as that used December 31, 1891, thinking there might possibly have been some unknown power of resistance in the first one. The first of them died January 6, 1892, at 10 A. M., and the other the next day. Pure cultures of the true germ obtained from both.

Thinking now it must be in the media, I tested all the bouillon in the laboratory made during my absence, and found it so acid, though not decidedly so, but not neutral by any means, that it was all thrown away, and new bouillon made under my personal supervision and the cultures from the guinea-pigs at once transferred to it.

That the decrease in virulence must have been due to the influence of the unsuitable bouillon is shown by the next tests of the cultures which were made.

April 5, 1892. Three field mice received one drop subcutaneously in the thigh. All three died on the morning of the 6th. Pure cultures obtained from the heart's blood of each.

April 30, 1892, 10 A. M. Inoculated two guinea-pigs with one-eighth ccm. of the thirty-third generation from the tick, including the animal transfers; bouillon transfers to new material every seventh day. Both died on the morning of May 1, and pure cultures were obtained from the heart's blood of each.

Certainly we have here sufficient evidence to conclusively demonstrate:

1. That one species, and one only, of bacteria can be isolated from the blood and organs of cattle diseased with the Southern Cattle Plague or Texas fever when done under proper precaution.

2. That the same micro-organism has been clearly demonstrated to have been present in the blood and faecal discharges of ticks taken from the bodies of southern cattle.

3. That the same organism derived from ticks can kill cattle when

injected into the blood, as well as cause a disease in them presenting both lesions and symptoms of Texas fever.

4. That the manure from such cattle was repletely infested with same organism.

5. That the same micro-organism is in the tissues of all the animals that died or were killed on account of this disease as a natural infection, as well as such as were killed by experimental inoculation.

I now ask every unprejudiced reader or competent investigator, is there a single link wanting or a fracture in the chain of evidence in any direction that the Southern Cattle Plague is not only caused by bacteria, but by the one here described? Is not the evidence in full accordance with the Koch postulates, even when adhered to in the most orthodox manner?

Do not these investigations completely demonstrate that the methods of the government investigators must have been in the mildest sense of the term unscientific and hence totally unreliable; and that their dictum, "Texas fever is a disease not caused by bacteria," is absolutely erroneous? That their final conclusion, "No ticks no Texas fever," must be equally so, though the ticks may or can, like the southern cattle, through their faecal discharges, infest our northern pastures and thus our cattle are infected?

The dictum must still read: Ticks or no ticks, the Southern Cattle Plague is caused by bacteria. Protozoa do not cause the disease, but if present, which is much to be doubted, are an accidental invasion.

Thus another "humbug," created by the investigators of the agricultural department, is blown to the winds. It is to be wondered if any of them will again dare to face the verdict of the scientific world with the assertion that:

"I am prepared now to say that Texas fever is not a bacterial disease. There are no peculiar bacteria to be found in the blood, spleen, liver or other organs. All the illustrations which have been published showing preparations of blood from Texas fever animals swarming with bacteria, and sections of tissues showing the same micro-organisms, are misleading and may be put aside as of no value to the student. The blood and tissues do not present such appearances when properly examined." *Sic!*

Dr. Paul Paquin, late state veterinarian of Missouri, and investigator of animal diseases at the Agricultural Experiment Station, made some investigations of Texas fever and published them in a bulletin dated May, 1890. I most cordially wish that I could speak more favorably than I can of this work of Dr. Paquin's, for I know he worked

diligently and honestly. That he differs from me somewhat does not bear on the case at all, because differences in opinion will sometimes occur to everybody. It is only those who maliciously attack my work, or interfere with my desire to serve the public, or those who ignorantly support them, for whom I have no mercy and who excite my ire to a degree that will not permit of the usual language used in such discussions. I will simply give a few brief quotations from Dr. Paquin's bulletin. The reader must take them at their own value. "Thirty-three different specimens from twenty different southern cattle of several counties in Texas and Indian Territory were analyzed. In all of them without exception were found, after a long and tedious search, germs that could be plainly seen and easily stained. * * * In the blood and bile germs were frequently found in a motile condition. They would roll on themselves, bend to and fro, move from place to place, etc.

"The germs of Texas fever are found in all southern cattle coming from infectious grounds, not only in their manure, but in their blood, liver and spleen.

"That the germs in fresh excretions of southern cattle remain harmless twenty to fifty days, and in a cool shady place much longer."

I sent Dr. Paquin the few remaining slides of my cultures and tissues while I was in Chicago, of which he writes in his bulletin. He thinks that which I call the mature form, of which I am certain I am correct, is not. Here is what he says:

"Dr. Frank S. Billings has grasped and explained better than any the significance of at least one form of the germ and understood much that investigators before him misunderstood, and has foreseen largely the possibilities and properties of the parasite. Judging from researches necessarily limited by time and circumstances, Dr. Billings concluded that in their mature form the germs presented a diameter of about twice that of their transverse, that they are very small, ovoid, and have rounded extremities.

"Pure cultures can be obtained from the liver, spleen, kidneys etc. of southern cattle of the infectious districts.

"The ticks full of blood from infected southern cattle may, it seems, scatter the germs on our lands, though ticks of themselves cannot convey the disease.

"With Billings I have found the germs motile, but only in certain forms. In drop cultures, as well as infected fluid or pulp from diseased subjects purposely killed, I have found this to be the case."

The trouble with Paquin's work is, that neither of his illustrations

have a picture of a pure culture of anything, though he notes the ovoid germ, called by me the mature form (the ovoid-belted pole stained bacillus), nor have any accurate descriptions of any one organism been given. In fact justice compels me to say, that he does not so describe anything as to warrant the conclusion that he ever had a pure culture of any one organism in his possession. We have not the briefest description of developments in and on different media by which to make any comparisons, so that his work is valueless regarding any specific germ, and only valuable in showing that bacteria are present in the blood of southern cattle, and by inference that he had under observation the same germ of which I sent him slides of cultures and tissues.

One thing surprises me, and that is, that he should have had any difficulty in cultivating these organisms, for none grow easier and at room temperature on all the ordinary media when exactly neutral, or slightly, though not strongly alkaline. That they are a little difficult to catch in solid cultures from the heart's blood is true, and that is what brought me first to the "blood cultures" made at once as I have detailed, and taught me their great value, but if a large number of freshly prepared agar tubes are inoculated, some will develop. In neutral bouillion, however, I have never known the heart's blood to fail when several good-sized drops have been directly introduced. In fact, I never think of going into the field unless supplied with my "blood-culture," bottles and bouillion flasks. The solid media are less reliable and can be used later in the laboratory, though I always take some agar tubes with me also, but do not always use them when it is inconvenient, preferring to taking a piece of organ with me wrapped as directed, and inoculate the tubes under cover. On this question of cultivation, Paquin surprisingly says: "No part of our work has offered more and greater difficulties than the artificial cultivation of the germs of Texas fever. Beef-broth, peptonized, solidified with agar or gelatine, or in a liquid state; blood serum, egg-albumen, potatoes, artificial lymph, and several other media have been tried. We have found that they develop best in a mixture of artificial lymph "with beef-broth" (p. 11). I am sorry to say that I must flatly contradict every word of the above, as it is as emphatically contradicted by all my experiences, except regarding the artificial lymph, which is an article I know nothing about. In that regard I would most earnestly warn any young investigators against "running after strange gods." If they ever read the Bible they will know where that will lead to. Stick to the natural media and the proven media, but, above all, study what natural media are and endeavor to imitate them all possible, for our artificial imitations will be

found unnatural enough. This curiosity hunt, to see what germs will do under the most unnatural conditions, has led to any amount of unnecessary confusion, and should be religiously avoided in my opinion. As I have so often reiterated, the only inviolable point of diagnostic value is that a given germ is invariably found in all cases of a well-known and long recognized specific disease, and be cultivated therefrom in any one medium, and the disease reproduced in the same species by inoculation. That is reliable testimony. Even though we cannot as yet cultivate the organism, but can diagnosticate it in the blood or a tissue, and with that blood or tissue again reproduce the disease in the same species, and again find the germ as before—that testimony is equally reliable.

THE PERIODS OF EXPOSURE AND INCUBATION IN SOUTHERN CATTLE.

In the investigation of any infectious disease, but more especially one presenting so many striking phenomena as does the clinical history of this disease in our northern climate, one must be very cautious and differentiate very sharply between what may be called the period of exposure and that of incubation. The former, exposure, must naturally be very uncertain and more or less influenced by external conditions; while the latter, incubation, must present quite constant characteristics, being only influenced by the receptivity of the animal organism to infection, the physiological adaptation of the tissues of its organism to the biological necessities of the inficiens proper, the germs, and the degree of virulent activity, toxic or disease-producing qualities, which the bacilli produce.

The period of incubation is the time which elapses from the first entrance of the specific inficiens into a susceptible organism to the time when the first physiological disturbances can be discovered, which in general would be a rise in temperature. It begins at once with the inoculation of the animals. In cases of natural infection we cannot control the clinical course of any disease with any such exactness, for the germs gain access to the animal body generally in small quantities and often at intervals, so that we know nothing of their presence until manifested by positive phenomena. In Texas fever, for instance, the period of incubation under natural conditions is probably but a few days, perhaps five or six, at least it is under fifteen days. Cattle have been seen to go into a pasture known to have been infested by southern cattle, and to have been first observed to be ill in twenty days from that time, as in a case at Tekamah, Neb., in 1887. The period of exposure, to illustrate by this disease, is the time elapsing

from which the southern cattle were first introduced to that in which the disease first appeared in the natives, a most uncertain thing, varying, as will be shown, from thirty-three days in the shortest instance, to ninety in the longest, and averaging 59.80 days. Dryness and temperature are the two primary factors exerting an influence on this period in the disease.

An Outbreak of Southern Cattle Plague in Northern Cattle, due to other Northern Cattle which were Infected in a Pasture Infected by Southern Cattle.

It has always been asserted that northern cattle infected with the Southern Cattle Plague through the agency of Texans or other southern cattle could not possibly play any part in the further extension of the disease to other northern cattle. A true statement of the conditions would have been to have said that no case of the disease had ever been known to occur in which diseased northern cattle had taken any part. It has been in my good fortune to have not only seen, but to have studied, such a case through its entire history from the original arrival of the southern cattle in the north to the primary outbreak in northern cattle due to infection on the pastures infested by the Texans, to a second outbreak caused in other northern cattle by the first natives infesting pastures after they were themselves diseased, on which no southern cattle had ever been and the second lot of diseased northern cattle had never been, nor any southern cattle. Notwithstanding that the government investigators have fitted their "tick protozoon" hypothesis to this case also by the probably correct assertion that tick development goes on until cold weather stops it, still there were no ticks on the cattle on which I made autopsies in the second outbreak. So much I know! Whether or not there were any on the balance of the second infected native cattle I do not know.

The assertion that diseased natives caused and did infest their own pastures and cause a second outbreak in natives met with the usual reception of any publication of the hitherto unknown or unobserved. It was, in general, flatly denied and contradicted as "impossible" the objectors forgetting that it is the "impossible" that is so often occurring and upsetting the most positive opinions of ignorance. This observation is, I think, the only one I have recorded which has not been contradicted by the Department of Agriculture; most probably because their "tick-protozoon" hypothesis was somewhat supported by it.

This second outbreak in natives occurred at Tekamah, Neb., in

1887. The cycle of events were as follows: Eleven hundred Texas cattle were imported into Nebraska on the 30th and 31st of March, 1887. The first indication of the disease in northern cattle was observed in twenty-one town cows which were placed in the pasture where the Texans had been, from April 1 to April 15, but were not at the time, after the grass had well grown, sometime about May 1, 1887. These cows began to die about July 1, and only one remained. Not a Texas steer was ever ill during the entire period. For these cows the period of exposure and incubation, taken as one, was ninety days, as recorded.*

Now as to the second outbreak: On Sunday, June 19, 1887. Twenty-four northern steers, which had never been near a Texan, nor on grounds where one had been, escaped from their pasture, some six miles from town and were wandering about, and for safety were thoughtlessly placed in a pasture where some 600 of the original Texans had been for about two weeks, that is from April 1 to 15. In this pasture these twenty-four natives remained, and grazed over night only. The next morning they were taken by their owners and again driven to their own pasture, six miles distant, where there were 114 of their companions—138 in all being in this field.

They were first found sick on July 9, and twenty-three of the original twenty-four became ill; two recovered. Period of exposure and incubation about twenty days. Twenty-one died.

No further deaths nor any cases of illness were seen in the balance of this herd (the 114 companions of the twenty-four which broke out of the pasture and were placed in that where the Texans had been) until September 21, when the owners found several dead in the field. This was just fifty-two days from the time the twenty-four steers were seen to be sick after their return to this pasture after being placed in that on which the Texans had grazed, or been, from April 1 to April 15. It will be seen how nicely this fact fits into the cycle of development of ticks, "from forty to sixty days," we are told.

Not a Texas steer had ever been inside the fence of this pasture in which this second outbreak in natives occurred.

The Texas cattle could not possibly have been the direct cause of this second outbreak in native cattle, though indirectly the cause of it, having been the vehicle by which the bacilli were brought from their native soil in Texas to the first pasture where the first twenty-four steers became infested with them.

We want some proof of this and can find it easy and at hand.

* For further details the reader is referred to the earlier editions of this work.

In neighboring pastures, but separated by two wire fences and a wide road, were two of the original lots of Texas cattle. They had been in this pasture since May 15. In one field were nearly six hundred of these Texans, and one native, which had been with them since May 15, and all had been well since that time. In the next field were two hundred Texans, and about sixty native yearlings, which had also been with them from May 15 to the 10th of August, when thirty of the yearlings were removed to another pasture. We shall refer to these thirty yearlings again soon. Now all these cattle, the two hundred Texans and sixty yearlings, remained well up to August 10, though two lots of native cattle which had been placed on pastures where the Texans had been from April 1 to 15 had previously died. The two hundred Texans and thirty yearlings left with them after August 10 also remained well. Again, other lots of the same Texans had been pastured with natives ever since the middle of May without a single case of disease occurring among either natives or Texans.

These facts, then, go to show that, from May 15, 1887, to the end of the grazing season, every possible opportunity had been given for native cattle to become infected through the agency of these Texas cattle and that the latter had been absolutely harmless during that time, though in the short time from April 1 to 15 they had profusely infested pastures, when no natives were among them, that were pestiferously dangerous to natives after the middle of May the same year, though not a Texan had been on these pastures since April 15.

These facts can be verified by any doubting person by suitable inquiries of Mr. Templeton and other owners of the Texans at Tekamah, Neb.

It seems to me that the above unquestionable facts in the history of these outbreaks of Southern Cattle Plague at Tekamah, Neb., in 1887, completely upset the "Natural-tick-ova-hatched tick-protazon" theory of the Agricultural Department.

What were the ticks doing all this time, from May 1 to 15, on some 1,100 Texans, among which large numbers of native cattle were pastured? Certainly the season of inoculation and hatching was the favorable one. On particular inquiry of Mr. Templeton, he assures me, that there were still ticks on the Texans, and some on the natives during this time, though the latter all remained free from disease in every form.

Again, I questioned the cattle owners around Tekamah most sharply, and every one coincided in most positively asserting that there was not a single case of infection in a native in or about Tekamah, that occurred

from any pasture, trail or roadway on which the Texans were, or over which they were driven subsequent to April 15, that is, fifteen days after their arrival in Tekamah.

Every place where they were prior to that date caused the infection of natives. This is especially true of the two fields where the Texans were herded from April 1 to 15.

All these facts not only put a quietus on the tick-protozoon theory, but most positively demonstrate that the Texas cattle could have had no direct connection with the second outbreak of the Southern Cattle Plague; that which was caused in the 114 natives on September 21, by the 24 natives of the same herd which got out of their pasture on June 19, and were placed where the Texans had been from April 1 to 15.

The period of actual exposure and infection was in reality nearly the same in the second outbreak as the first, when we consider that the town cows were not put into the pasture where the Texans had been until about May 1, and in this case the time which elapsed from the exposure on the pasture to the actual eruption of the disease must have been somewhat larger for climatic reasons. We shall return to this topic.

This outbreak in the 114 native steers, due to their twenty-four companions having been placed in the field where the Texans had been placed from April 1 to 15, is, fortunately, not the only evidence at hand of a second outbreak of this plague being caused by natives infected by the primary agency of Texans. Another occurred at the same time and place, at a distance from the first one.

A SECOND OUTBREAK IN NATIVES DUE TO NATIVES AT TEKAMAH, NEB.

During the July outbreak Mr. Templeton suffered more severe losses than any of his neighbors from the infection of his pastures by the Texas cattle, of which he was the largest owner. His loss was mainly in a herd of eighty cows. Thinking he could put a stop to the disease by removing the cows not yet sick from the field where they were, in the early days of the outbreak he took fifty of those remaining from the infected pasture and placed them on one distant and secure from all danger of infection from Texas cattle and watched them very closely. Nearly all died! The dead ones were drawn off out of this pasture and buried in a deep gulch at a distance, where they were well covered. By the end of July this outbreak terminated and the pasture contained but a few recovered cattle and some horses, in which condition it remained until August 10, 1887.

Allusion has previously been made to a bunch of two hundred Texas cattle that had been pastured with sixty native yearlings from May 15 to August 10, and that not only up to that date they all were well, but also that from August 11 to frost the Texans and thirty yearlings remained together and were free from disease. On August 10 Mr. Templeton took thirty of the sixty native yearlings and put them in the pasture where his isolated cows had died, and which had been free from disease for about two weeks, as stated above. He thought he was perfectly safe in doing this, as it was the general belief that the disease could be no further extended through infected northern cattle. The few animals on this pasture were not sufficient to consume the grass and so the thirty yearlings were taken from their companions and the two hundred Texans and placed on this pasture where the cows had perished, and where no Texan had ever put hoof or dropped manure, and hence no "ticks" and the protozoic accompaniment were possible, unless brought by the thirty yearlings themselves, but as their thirty companions still remaining with the Texans remained free from disease, we can dispense with "tick" in this emergency.

These thirty yearlings were "Polled-Angus heifers. They were removed to this pasture all well, on August 10, 1887, and on September 1, or twenty days after, they were first observed to be ill and quite a number died. Here again we have a period of about fifty days from the time the original lot of native cows primarily infected through the direct agency of the Texas cattle to the first cases of illness observed in second lot of native cattle.

These experiences show that under favorable climatic conditions about fifty days must elapse from the time southern cattle arrive in the north before native cattle can be expected to become infected, and that where a pasture has been already infected, and time enough elapsed for the distribution of the germs at a favorable time of year, it takes about twenty days before native cattle exposed thereon begin to show serious signs of illness and deaths to occur.

THE MANURE OF SOUTHERN CATTLE THE CHIEF MEDIUM BY WHICH
THEY INFECT OUR NORTHERN PASTURES AND THROUGH WHICH
OUR NORTHERN CATTLE BECOME INFECTED.

In the preceding pages the fact that the Southern Cattle Plague is caused by a bacillus has been completely demonstrated. It has also been shown that that same bacillus can be in the faecal discharges of ticks taken directly from southern cattle, as well as in the internal fluids of the ticks. The same bacillus was also found, not only by plate-cultures,

but by the inoculation of a guinea-pig, in the manure of one of the calves, into the jugular vein of which a pure culture of the bacillus from the tick had been injected. These unquestionably demonstrated results should indicate the chief manner, not only by which southern cattle infect our northern pastures, but also, if the ticks do play any part in the drama of this American cattle plague, that it must also be through sucking the infected blood from southern cattle and by their own (the ticks') discharges, also infect our northern pastures. Though there are some mysteries in the reported investigations of the Agricultural Department, especially in the case where the young ticks which were artificially hatched from ova and placed on northern cattle, said to be so placed that there was no possibility of their deriving the disease in any other way than through these young ticks, still we must ever bear in mind two facts:

1st. The long-continued and well-proven unreliability of all such statements emanating from that source.

2d. The absolute improbability, practically an impossibility, of the tested cattle ever having derived Texas fever in the manner claimed, especially when we also take into consideration the admission of the Agricultural Department that while "these brief notes demonstrate that Texas fever can be produced by placing young ticks on cattle, and that the disease cannot be due to any abstraction of blood, for the ticks are quite small and have scarcely begun to draw blood on a large scale." (Report of Bureau, 1889-90, p. 98, l. c.)

I say, with that admission and the other improbabilities of this etiological fairy tale, there must have been some other factor at work of which the investigators of the government make no mention.

That that factor could have been the faecal discharges of southern ticks goes without question. That it could have been the same of southern cattle is also equally plain. That it could have been anything else is not probable and scarcely possible.

It will be remembered also that we have no mention of a single attempt having been made to test either the blood, organs, or manure of any of the cattle mentioned, or the ticks, for any micro-organisms whatever, which shows most blamable neglect and a totally unscientific method in conducting these investigations. The positive results of another investigator cannot be negatived until others have repeated his work in every detail and under exactly the same circumstances, and fact by fact disproved it. It will not do to "come at me" after these words are published with the *a priori* assertion that such investigations were made. I know those persons better than that. They may have

been, it is true, but had they fallen out negative as regards the presence of micro-organisms, the circumstantial evidence of their malignant desire to injure the work of this laboratory is of so positive a nature that no unbiassed observer will believe for a moment that they did result negatively, and no mention made of the details of proof and how it was obtained.

Quite the contrary is warranted by all the facts connected with the department investigations.

Those people in Washington are "very clever," as our English cousins say; "very sharp," as we Americans express ourselves. This had been exemplified in many ways, which, while looked over in some circles of society, certainly are unpardonable in a department of government created by the people for their own benefit. The accidental presence of a micro-organism of the same species of the German swill-milk disease in hogs, mistakenly called "*Schweine-seuche*," was a most fortunate "find" for the government investigators, and the most "sharp" practices resorted to force a second wide-spread "swine-plague" on this country, as an offset to the growing appreciation for the Nebraska investigations, especially in the endeavor to show the futility of inoculating against the true swine plague alone. This "trick" having failed to win, the next thing we experience is the wolf-cry of the terrible dangers of inoculations, which will eventually have the effect that the "wolf" did on the bad boy. Now we have the tick-*protozoon* hypothesis, if we dare to honor so absurd a statement even with that word, as the cause of the southern cattle plague, hoping thereby to again nullify the value of our work in the minds of the stock-raisers of this country; and, finally, the Corn-Stalk Disease is to be reported as a Nebraska dream! Ask the farmers who have lost millions if it is a laboratory fantasy or not?

It is singular how fortunately events sometimes turn out for people of that character. The Loeffler-Schuetz work gave them the foundation for a second swine plague, and the accidental discovery of a similar germ which could only be surely discovered by rabbit inoculation gave them the necessary powder. Laveran's and others' work supplied the *protozoon*; Koch supported it, and either the *a priori* assumption of its presence in the red blood cell of the southern cattle, or some process of necrosis of the plasma of that cell (it is a well known fact that the cells disintegrate in this disease), gave color to the assertion.

Another most accommodating circumstance was the real correspondence in the cycle of development from old ticks, through ova, to

young ticks, the time required corresponding very closely with the period averagely elapsing between the arrival of southern cattle on our northern pastures and the eruption of the disease in native cattle.

They tell us that "it requires from forty to sixty days for the matured ticks to drop from southern cattle and the eggs laid by them to develop into young ticks" (l. c. p. 97), a most fortuitous circumstance which fitted very nicely into the results of my previously published investigations.

OUTBREAKS OF TEXAS FEVER IN THE NORTH DUE TO SOUTHERN CATTLE.

Place and Date and Arrival of Texas cattle.	Date of Outbreak.	No. of Days After Arrival of Southern Cattle North.
1. Warren Co., Ind., June 12, 1868	Aug. 4, 1868	52
2. Warren Co., Ind., June 12, 1868	July 29, 1868	46
3. Odell, Ill., June 25, 1868	Aug. 10, 1868	47
4. Northfield, Ohio, July 4, 1868	Sept. 1, 1868	57
5. Farina, Ill., May 10, 1868	July 15, 1868	65
6. Tolono, Ill., June 25, 1868	July 28, 1868	33
7. Sodora, Ill., June 1, 1868	July 28, 1868	56
8. Champaign, Ill., June 15, 1868	Aug. 3, 1868	49
9. Tekamah, Neb., June 29, 1887	July 1, 1887	90
10. Roca, Neb., June 29, 1887	Sept. 1, 1887	63

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Average period between date of arrival and first notice of disease in northern stock, 55.80 days.

It is at once to be seen how closely this average corresponds to the average time for the cycle of development in ticks to occur as stated above. On the other hand, while I positively know nothing about it, it does not seem exactly reasonable to suppose that in the cycle of tick development we should have the extreme variations shown in the outbreaks of the disease following the arrival of southern cattle on our northern pastures. It does seem to me altogether impossible for the tick-ovulation-protocoon theory to conform to the actual facts occurring in this disease, while the fæces of both cattle and the ticks on them, lodging the germ and protecting it so far as cattle manure is concerned, are conformable to the entire phenomena of the development of this disease in northern stock. While I cheerfully admit that the southern ticks direct from the cattle can possibly supply fæces enough charged with the bacilli to play some part in causing this

disease in northern stock, that they do not play any essential role in any way, seems supported by the one peculiar clinical phenomenon in this disease, which is, that it has never been known to extend across, under, or over a wire fence to cattle on the other side, though killing those in the infested pasture with wondrous rapidity. This fact requires some exact observation with reference to the ticks. I have seen such an outbreak when the apparently unaffected cattle in an infected pasture were actually touching hair and licking cattle in an adjoining field. I have myself made an autopsy on a steer which in dying fell against the wires and was half through such a fence, yet the disease never went to the other cattle. I know the diseased cattle had some ticks on them. The trouble is I did not then think to study the tick question on the other side the fence, not being much inclined to "straddle a fence" on any question. Still it does seem to me as if under such circumstances ticks should pass from the cattle in one field to those in the other, and that did the ticks play any essential part in the etiology of this disease, it sometimes would have passed the boundaries of a wire fence. Had it ever done so we should certainly know it in this country.

THE SALIVA-URINE THEORY IN THE ETIOLOGY OF THIS DISEASE.

The hypothesis that the bacilli of the southern cattle plague were chiefly dispersed on our northern grazing grounds by means of the salivary drawlings and urine of southern cattle has been most strongly set forth by Dr. H. J. Detmers in the report of the Department of Agriculture for 1884. These media of infesting our pastures need no serious consideration, as neither of them have consistency enough to protect the bacilli against the sun's rays and other unfavorable influences. Were they the acting media of transplantation we should see infection following almost immediately on the advent of southern cattle in the north. That this has never occurred in less than thirty-three days, so far as we know, and generally requires over fifty, has been conclusively demonstrated. With the mature-tick-ovulation-artificial-hatching-protocoon-invasion-process we have nothing to do, having completely exploded it. We have then to consider in more detail

THE MANURE OF SOUTHERN CATTLE AS THE VEHICLE OF INFECTION.

As already shown, the bacillus of this disease was in the faecal discharges of the ticks and the inoculated calf. Were the germs not in the manure of the southern cattle when brought to our northern pastures the danger of southern cattle plague in our native stock would be

comparatively nothing, for even though we may admit that the fæces of the ticks do contain the germs, they do not offer the necessary protection to them which the vicissitudes of the weather in early spring make necessary to preserve their vitality. That it must be through their manure that the southern cattle chiefly infest our northern pastures is most strikingly exemplified in the Tekamah outbreaks. Up to the time of this outbreak, and another at the same time due to southern cattle shipped at the same time and from the same place into Illinois, which also caused an outbreak in that state, the period of safety in which southern cattle were permitted to be shipped north for grazing purposes was fixed at March 31, it being supposed that up to that time the weather was cold enough to prevent any danger to northern stock. The general idea has always been that "the first frost put an end to any danger of infection to northern stock. The experiences at this time completely exploded that idea and the time at which importations must cease was put back to either March 1st or February 1st, I do not know which at this moment, and it matters not in the case, as I only desire to show that we had most severe freezing weather after April 1, 1887, and yet it had no effect on the future outbreaks of the disease, and to show also that it must have been the manure which protected the germs, and hence the outbreaks. On the shipment of cattle into Illinois at the time mentioned, the following letter is very instructive:

"DEAR SIR: Yours to our Mr. McChesney is at hand, also a copy of the *Nebraska Farmer*, with your very interesting article on 'Texas Fever' which we carefully read. We at once tried to learn the facts about the shipments of Texans, as mentioned by you, into this state. We found that a large lot were received from the region of country named, but could not learn if they came from the same parties. These cattle were a mixed lot, Texans and Shorthorn grades. A part of them were sold and taken from the yards March 30 to a farm twelve miles away. During this time the weather was cold, ice being formed at night. They were driven to Kankaku county. On the first farm they stopped over night Texas fever appeared July 16. The native cattle were turned into the pasture used by the Texans May 10. Several died and post mortems made by our veterinarians established the fact of Texas fever. All along the track to point of destination the same disease appeared. The owner states that none of the Texas lot have been sick.

"Very respectfully yours,

JNO. M. PIERSON,

"Chairman Live Stock Commission."

The exact correspondence in every detail with the results at Tekamah, Neb., the same year, must be apparent to every one who has read these pages carefully. The 1,100 southern cattle arrived at Tekamah, Neb., March 30 and 31, and every one remarked that it froze hard every night for two weeks subsequently, which fact was frequently quoted to me, and has since been repeatedly talked over as having given the people a feeling of perfect safety as to any dangers to their native stock from the Texas cattle. As said before, every cattleman in the north believed that "the first frost would kill Texas fever," but no one had thought that that "first frost" which had always been noticed in this connection came in the fall of the year to be followed by constantly increasing cold weather, and not, as in this case, in the spring with the warm weather rapidly approaching, and the sun undoing the work of the frost every day.

Had the germs been in the saliva or urine of the Texas cattle and not destroyed by the frosts, the disease must have broken out in some of the native cattle much earlier, as it only takes about twenty days to cause it after a replete development of the germs in the pasture has taken place.

Something must have protected the germs from the outward action of the frost during the cold nights of those two first weeks of April, 1887. What was it? It could have been nothing else than that they were locked up in the manure of the Texas cattle dropped in pads on the pasture. So long as the pad of manure dropped by the infected cattle—Texans or natives—remains moist, soft, and cohesive, there is no danger to native cattle placed on pastures where such infected cattle have been. The results at Tekamah demonstrate that fact with positive certainty. The second outbreak emphasizes it much more sharply than the first; for the climatic conditions were favorable all the time between the first and second outbreaks—from July to October—yet there were only twenty days' actual variation in the time in which the eruption took place if we carry the first outbreak to a time when climatic conditions made infection possible in native cattle.

The freezing of the surface of the manure pads in the early days of April helped put them in a condition favorable to the preservation of the germs within them. It made a hard, close crust on the outside of the pad of manure, which was also increased by the evaporation caused by the ever increasing heat of the sun in the daytime. These two factors together also materially aided in increasing and supporting the temperature of the inner portions of the pad, thus favoring the life and increase of the germs. A careful comparison on plates made be-

tween the number of specific bacilli in the old pads of manure and those in the fresh manure taken directly from the large intestine of a killed animal demonstrates a far greater number of the Southern Cattle Plague germs in the old pads in comparison to the intestinal manure. That these germs will keep their vitality for a long time in such manure when protected from evaporation and extreme temperature variations is demonstrated by the fact that in the summer of 1887 I took some of the moist contents of the inside of one of these pads dropped by the Texas cattle and tested it then, as above, with fresh manure, and again in March, 1888, and March, 1889, both by plates and tests on ground squirrels, and even to the last time the germ did not seem to have lost in virulence in the small animals. Thus it will be seen that as the time elapsed and these pads of manure became older, they must have become a most dense culture of the specific germ, and when in May, by means of the elements, they had become broken up and distributed in the grasses of the fields, a most general infection must take place as soon as the warmth was sufficient, the growing grass protecting them from the direct action of the sun. Only after the pasture had been thus prepared were the natives thereon—the twenty-one cows—really exposed to infection, and not before, no matter how long they may have been there previously. This accounts for the long time, ninety days, elapsing between the arrival of the Texas cattle at Tekamah—April 1—and the first outbreak in natives—July 1, 1887—and explains why the frost did not kill the germs.

In the second outbreak, where the twenty-four native steers infected their own pasture, leading to the infection of their companions in September, the same cycle of events had to be passed through. The twenty-three infected cattle were in and of themselves not the least dangerous to their 114 companions, which had remained in the pasture while the others were wandering around and confined in the pasture infected by the Texans, though for one night only. They, as cattle, were not dangerous. This fact is recommended to those investigators who insist that "contagious," in the hygienic sense, is the same thing as transmission by inoculation. The Southern Cattle Plague is transmissible by inoculation. They were vehicles for the conveyance of a vehicle, their manure, in which was the specific cause, the bacillus of the diseases from the pasture infected by the Texas cattle, to their own, in the same manner as the southern cattle were also vehicles of the same vehicle, and the same germs from the grazing grounds of Texas to those at Tekamah. The manure of those twenty-three infected cattle was the shell of the bacillary-dynamite which was later on

to explode in their 114 companions. But when this manure was first dropped by twenty-three sick natives it was not dangerous to the 114 others, or we should have had infection in twenty days, as in the twenty-three which were confined in the thoroughly prepared (to infection) pasture, where the Texans had been from April 1st to 15th. It was not dangerous so long as the germs were confined in the solid and unbroken pad of manure as dropped by the infected cattle. As before, the germs were constantly developing in the pad, and directly from the time it was dropped, in a most rapid manner, because the climatic conditions were all the time favorable. There were no cold, frosty nights and cool weather to check the operation. The drying up of the pads also took place much more rapidly, and there were 114 cattle in the pasture walking through them and distributing the manure in small fragments, aided by the elements, in the roots of the grasses, thus innocently preparing the way for their own funeral. Not until a general dissemination of the germs had thus taken place was there any danger to the 114 other natives. They grazed on these death-secreting grasses in blissful ignorance of the wrath to come, which occurred in about fifty-three days from the time their twenty-four companions were returned among them from the Texas-infected pasture.

This time appears unnaturally long when we consider that the climatic conditions were far more favorable to the rapid development and dispersion of the germs in the grasses than in the first instance; but my observations teach me that the germ loses some in virulence in passing through cattle, and that it gains it again gradually when developing in earthy material. The southern cattle came north with their intestines freshly loaded with the germs from their native soil; they remain in these cattle some two weeks before their intestines become thoroughly cleared of their southern food, as shown conclusively by the experience at Tekamah, where the Texans arrived March 30 and 31, 1887, and every place they went previous to April 15 became afterward pestiferous to natives, while the places where they were subsequent to April 15 were perfectly harmless to native cattle. The tests made with the germ from my inoculated steer taken from the Roca cattle, both in the first generation, showed a loss in virulence in the inoculated steer in comparison with the first culture from the Roca animals directly infected from the pasture infected by the Texans. With these remarks I think that the bacillus of the Southern Cattle Plague has been most exactly demonstrated, as well as that the manure of southern and infected native cattle is the chief medium for the transference and protection of the germs by which our pastures are

infected, and cattle infected with this, the most virulent and malignant of American cattle diseases. It has also been shown as conclusively, I think, that if ticks do play any essential part in the etiology of this disease, that it must be the mature tick coming north on the southern cattle which suck the germ-infected blood from the southern cattle and pass the bacilli off in their fæces to the ground, and not the young ticks hatched from ova after the arrival of the southern cattle in the north.

NATURE OF THE SOUTHERN CATTLE PLAGUE.

In considering this question it will be impossible to avoid the repetition of some things which have previously been said in the consideration of the etiology of the disease, but there seems to be such a vast amount of ignorance on this subject, such a complete neglect of sound reflection and every logical sequence in the minds of many investigators and more practitioners, that such repetition is not only pardonable, but demanded. To decide correctly as to the primary nature of a given disease seems to be a most complicated question to many, whereas with competent education, acute powers of observation, and logical, inductive abilities it should be a very simple matter.

The pathology of an infectious disease naturally differentiates itself into two distinct yet directly related fields of observation.

1st. The primary origin of its cause, the *causa sufficiens*, the inficiens proper.

2d. The direct and indirect action of that inficiens in the infected organism; the pathological anatomy, the primary and secondary lesions.

With the second, the pathological anatomy, we shall have little to do at this time, as it has been treated in the necroscopical notes detailed in the etiology. Again, the Southern Cattle Plague is so acute an infection that the pathological anatomy presented is generally a very clear picture, unclouded by secondary complications, which renders that task much easier than in the study of diseases of a slower type and hence much more liable to secondary complications, such as the swine plague and corn-fodder disease in chronic forms.

Every well-read and reflecting student of medicine, particularly such as have studied works of hygiene, published at any time previous to the past decenium, well knows that the generally accepted expression of the writers was and still is, though we use other terms to express our conclusions:

"That while all contagious diseases are infectious, that all infectious diseases are not contagious."

Thus was and is a known and universally admitted difference indicated.

Many modern writers, but especially a small army of unintelligent imitators, have endeavored to deny this absolutely practical differentiation, by asserting that the words contagious and infectious were identical in meaning. I do not care an iota as to the sanctity which antiquity is often assumed to give to a statement or conclusion, any more than I do to the weight a great reputation is often assumed to give to a so-called authority. Positively and without reserve, I am my own authority, and so is every other man who has any opinion worthy of a moment's consideration. Infallibility is by no means claimed in that statement. We are all liable to error. Nevertheless, after weighing all the evidence, our own conclusions must be our infallible guide in all matters. The great trouble is that the majority of the human race have no evidence of their own by which to weigh that of others. They blindly accept the "has been" because of the supposed sanctity of age and its authors. Those who can observe acutely the phenomena of nature and draw conclusions that will stand the test of time have been and still are rare productions in the march of human progress. Whether right or wrong in my conclusions time alone will tell. One thing is sure, that is, that as every other man, I have no other light than that of my own intelligence to guide me. The work of others can be but a reflected light by which each of us may strengthen himself in the pursuit of knowledge. Unfortunately, only too many find the greater portion of their intellectual light in the transmitted reflections of the past. As the heat of the sun of yesterday aids that of to-day in developing our growing harvest, so the reflected intellectual lights of the past add warmth and strength to our own endeavors; but as yesterday's sun also adds strength and vigor to the weeds which we cut down and they dry and wither in to-day's sun, so the light of our ever increasing intelligence must cut down the weeds in the transmissions of past authorities and cause them to wither on the wayside of human progress. He who does this lives to-day, though he dies tomorrow. He is no disrespecter of the great authorities of the past. Their reflected light is the oil which makes the lamp of his own intellect burn all the brighter. The wick of past authority constantly needs trimming. Only the lamp of universal truth, filled droplet by droplet with the oil of knowledge, drawn from nature by the laws of investigation, burns constantly on the altars of human advancement. The flame has never yet gone out, though at times the wick has become thickly crusted with accumulated ignorance, or bitter errors, due

to the sanctity which unobserving man has had for dead authority. But still the flame flickered and gave some warmth, and anon comes an Aristotle, a Hippocrates, a Galileo, a Luther, a Boerhaave, a Hunter, a Bichat, a Newton, a Darwin, a Virchow, a Koch, a Pasteur, a Franklin or an Edison, and with one fell stroke the crust of superstitious reverence for authority is knocked heedlessly to one side, and again the lamp of truth burns and its warmth radiates through the nations, once more replenished with fresh droplets of the oil of knowledge wrung from nature's fountains by the intellectual giants of a living present.

Such has been, is, and will continue to be, the history of the medical as well as all other branches of human knowledge.

Beware! Let us not bow down in holy reverence before any "strange god" erected by our own ignorance and assert he and he only is an authority. Every man must be his own authority unto himself. Otherwise the world would die of intellectual dry rot ere another ten years had passed over our heads.

It is this "intellectual dry rot," this respect for and leaning on some other man's light rather than our own, which is the cause of all this confusion as to the natural classification of infectious diseases. I use the word "natural" purposely. All artificial classifications are but "intellectual dry rot." Read the history of medicine and the great artificial "platforms" on which diseases have been piled up in classified order in the past, if any reader doubts. Science is not art. Learn that lesson once for all and you will learn the first great principle necessary to become an investigator of natural phenomena. Science is method. Science is the methodical study of nature's methods, in order to apply them to human necessities. If the result be artificial it fails. If it be a simple, direct repetition of nature it succeeds and becomes a beacon light to further research in the same direction. The method of application is the art, but not the method itself. If the method is natural, then success follows. If artificial, it is unnatural, and failure, partial or total, results. That is a repetition, but not an unnecessary one. "Look more to nature than to the authorities" should be the rule of all true students. That is the inductive method which has led to all our modern acquisitions of knowledge, and the gradual adaptation of natural forces to our necessities. The deductive method may be said to be the "weeder" that subjects the remitted results of the past to the living sun of the present. Only the true grain stands the tests.

With a constant eye on the records of the past we as constantly press forward in the search of nature's truths.

The differentiation of the past, that "while all contagious diseases are infectious, all infectious diseases are by no means contagious," indicating two great classes, had and has to do with the primary origin of all infectious diseases only, and not with their clinical course, or the relation existing between healthy and diseased individuals of susceptible species.

The "contagious diseases" of earlier writers are now distinguished as "obligatory parasitic" or "endogenous" diseases—that is, diseases in which the specific cause, the *iniciens* proper, invariably finds its present source, so far as we have any historic record of the fact, in some one species of animal life, the chemismus of which's tissues is such as to offer the essential elements necessary to the life of the germs of such diseases.

Examples: Syphilis, contagious pneumonia, rinderpest, the spotted typhus, and the specific eruptions of man, scarlet, measles, and other diseases of like origin.

The diseases named above have also this peculiarity, that their possibilities of natural transmission are entirely limited to the species of life in which they primarily occur. There is another class of endogenous obligatory parasitic diseases which are not thus closely limited to one distinct species, but are transmissible, generally by accident, to one or another species. Such are glanders, rabies, the foot and mouth disease of cattle, and variola. More of this class are found in the domestic animals than in man. More animal diseases are possible of transmission to man than diseases of the human species to animals by natural accidents.

The susceptibility of the individuals of any species of animal life to the action of any specific *iniciens* is determined in both fact and degree by the favorable or unfavorable qualities of the nutrition offered the specific germs by the chemismus of the exposed or infected organism. Where the chemismus is unfavorable, no infection results. The pathogenic quality of micro-organisms is, under natural conditions, far more decided by the chemismus of the exposed organism than by any quality of the germs themselves, though the latter cannot be entirely disregarded, for certain unknown, known, ascertainable, or experimental influences can so alter the character of specific germs that for the time they may be unable to "manufacture" toxic or disease-producing principles from the food offered them by certain organisms, but frequent and repeated transmissions through such organisms, or ex-

perimentally in certain food, will result in the germ in question acquiring its pristine virulence, or in some cases increasing it, while in others it is decreased, and finally lost altogether. It is now an admitted fact that the specific germs of non-recurrent disease produce both a toxic, or disease-producing, and an immune-producing, or preventive, chemical principle. It is also becoming evident that the so-called "curative," or resistant principle, by which infected individuals survive, is the result of the action of some specific organ of the body, which on irritation produces a greater quantity, and that this chemical product of the infected organism unites with the toxic product of the germs and thus forms a new and non-irritating or non-disease-producing substance. When this product and combination takes place with sufficient rapidity, so-called recovery results. It is a sort of modern return to the dyscrasis and crisis theory of Hippocrates with an entirely different application. It is still an open question, to my mind, if all forms of germ, as every form of life, do not also produce another chemical product which in and of itself is eventually capable of being the cause of the death of its own producers, and which, when we can produce and isolate it in sufficiently concentrated quantities, can be also of use as a therapeutic agent with much more certainty than the antagonistic product of any organ of the infected organisms.

The protective-preventive vista of pathogenic investigation is only dawning on us at present, but the promised possibilities are most encouraging.

FACULTATIVE-PARASITIC OR EXOGENOUS INFECTIOUS DISEASES.

Directly opposed to the obligatory-parasitic endogenetic diseases are the facultative-parasitic or exogenous diseases in their primary origin. That is, diseases in which the *causa sufficiens* or *iniciens* proper as invariably finds its primary origin in conditions offered by the external surroundings of animal life as the endogenous causes do in those offered by the animal organism itself.

Here, again, the relation between diseased individuals and healthy ones of susceptible species the possibility of transmission has no bearing whatever on the differentiation. Here, again, nutrition decides the point of infection, but with this difference—the germs find their natural conditions favorable to their existence in external conditions, soils, etc., but where any species of animal life also offers a similar chemismus the germs have the faculty of living in their bodies, when introduced, and becoming specifically pathogenic; that is, disease-producing.

We must distinguish most sharply between naturally pathogenic and experimentally pathogenic, as has been pointed out in another place. The investigators of the Agricultural Department have done their utmost to confuse the farmers of the country on the points here discussed using the word contagious constantly in reference to swine plague, which is a plain facultative-parasitic or exogenous disease, and not a contagious one as asserted.

The mystifying language has been used in connection with the Southern Cattle Plague, where they say:

"There is no doubt that it is a difficult matter to understand how it is possible for the native cattle of a section permanently infected with a contagious plague to resist the influence of the contagion with which they are surrounded. It is equally difficult to understand how apparently healthy cattle can distribute contagion for so long a time after they leave the infected districts. It is equally difficult to understand why the cattle really sick (northern) of this contagious disease do not convey the contagion to others." (Report 1883, p. 20.)

The careful reader of these pages should easily understand how all these things occur. In the first place, a more striking example of a facultative-parasitic or exogenous disease, hence not contagious—does not exist. The transmission is through and by the manure of the infected animals and in no other way. It is easy enough for an educated and competent intelligence to understand why the southern cattle do not themselves show manifest signs of disease, though it is now admitted that many of them do die of it in their native country. John Gamgee said:

"In the south splenic fever is distinctively indigenous, and so far as Texas is concerned, I have satisfied myself that the disease is universally prevalent in the state." (Report on cattle diseases, 1871).

Again: "What are the active causes in operation which tend to influence the stamina of southern herds? Traveling over the prairies no one can fail to be struck with the large number of dead animals to be met with. The dissection of these animals, or the slaughter of the first animal met with, reveals their distinct and unfavorable manifestations." (Ibid, p. 107.)

Gamgee conclusively shows that Texas cattle do have the southern plague; though the majority do not die of it, and it is a part of that majority which comes north to us. That they do not die is due to the fact that they take in the germs in small doses from the time they begin to graze while sucking calves, a period when all herbivora are less susceptible to diseases of this character than at any other time in

their lives, and before they are harmed they have acquired immunity against further disease-producing powers in the germs, with which still their bowels are completely filled and which they bring north with them when imported, and infect our pastures as already fully explained.

There is nothing difficult to understand in the whole matter to any person having a healthy and active understanding.

The influence of Laveran's observations in fever and ague, and Koch's conclusions as to malarial diseases, necessitates that we again briefly define what a malarial disease is.

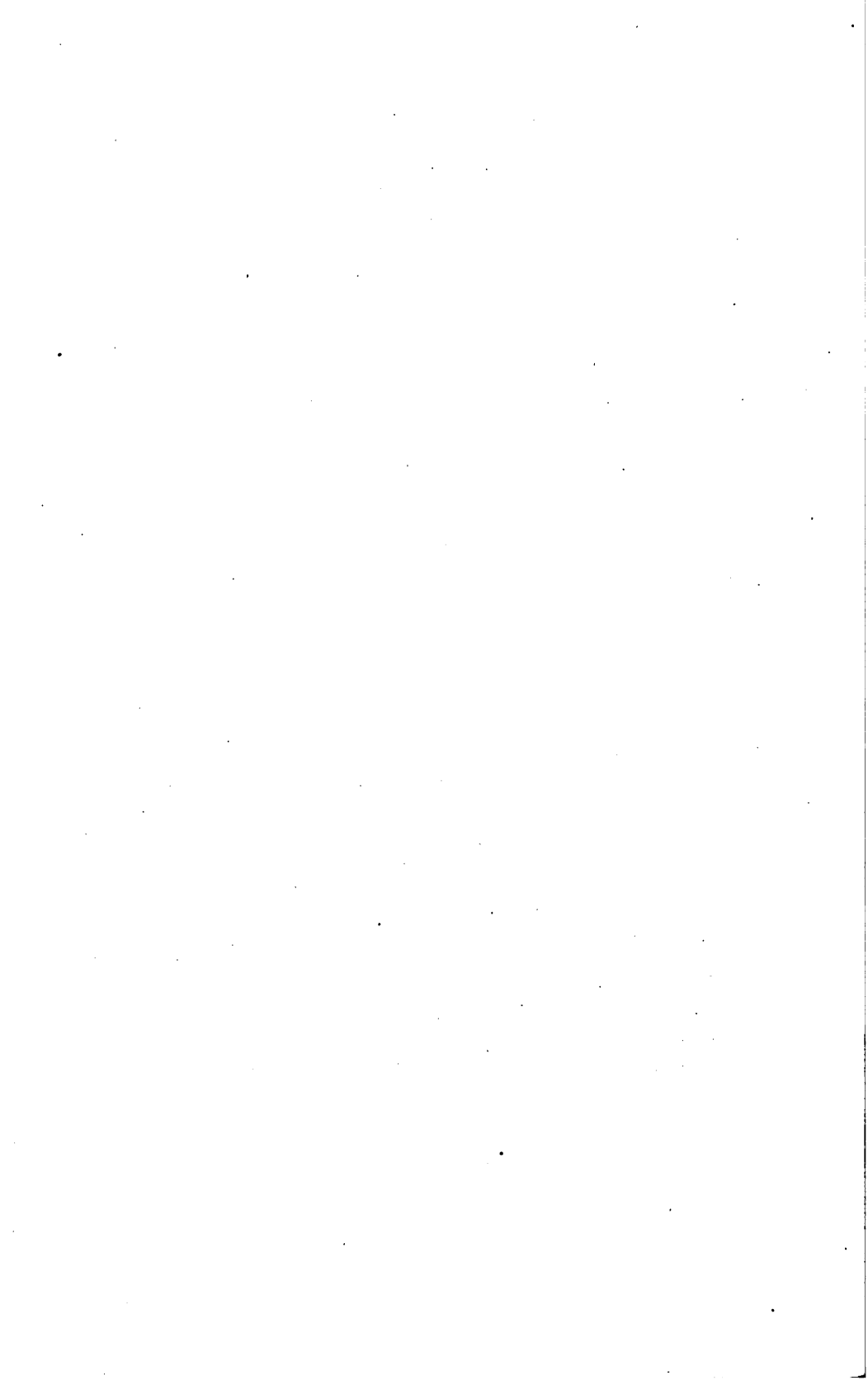
It is an exogenous facultative parasitic disease of a most local character, being neither naturally transmissible with any species of animal life, nor transportable from its place of primary origin by a diseased person, or any secretion or excretion from such a diseased individual.

It is self-evident that the Southern Cattle Plague is not a disease of such a character. Its local origin in the soil of certain sections of the south is as marked as is the transportability of its inficiens by cattle from such localities to our northern pastures, and the infection of our native cattle.

How native cattle can be the means of extending the disease further to a second lot of natives, has been fully explained. All that is necessary is that the original lot of southern cattle come north early enough to guarantee a prolonged enough existence of suitable temperature for the full order of events to occur in the manner described.

As a pathological entity a study of the necroscopical notes accompanying this paper, and the clinical symptoms of this disease are sufficient to demonstrate to any competent person that the Southern Cattle Plague is a specific septicæmia; a blood poison due to the development of a specific irritant—the bacilli—in the blood from the intestines, and the nutrition of, by the blood of the bovine organism, being favorable, the germs thereby secrete, produce, the toxic or disease-producing principle which gives to the disease its specific characteristics in its clinical course and pathological phenomena.

This is no new idea, as the same conclusion was arrived at by the investigators of the Metropolitan Board of Health of New York, and published in the transactions of the New York Agricultural Society in 1868, p. 1109.



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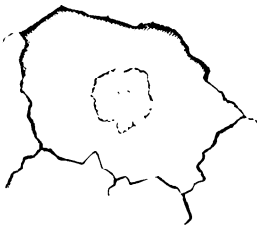
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PLATE I.

FIG. 1.



FIG. 2.



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FIG. 3.

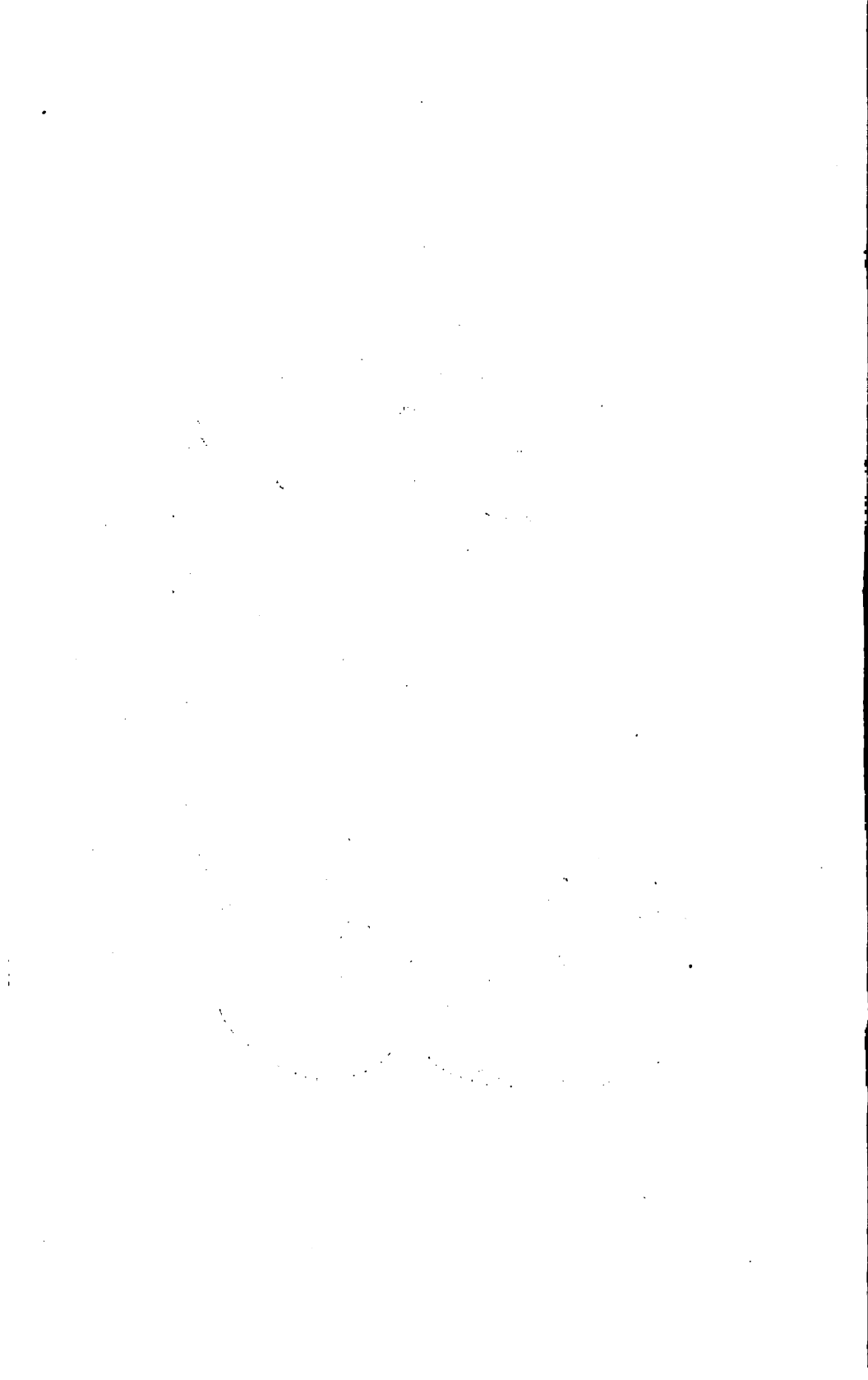


Figure 1, Texas Germs on Potatoes.
Figure 2, Texas Germs on Eggs.
Figure 3, Texas Germs in Gelatine.

PLATE II.



Diffuse Capillary Engorgement of Kidney in Southern Cattle Plague.
From U. S. Agricultural Report 1871.



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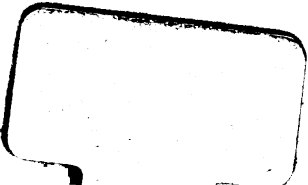
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